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IENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

# *The* Journal *of* Parapsychology

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This JOURNAL is published on the thirtieth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $6.00 a year; single current numbers $1.50. The rate for back volumes is $7.00; for single numbers $1.75. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

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Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

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DUKE UNIVERSITY PRESS

*The* Journal *of*
# Parapsychology

VOLUME 22 MARCH, 1958 NUMBER 1

## CONFIRMATION OF THE PK PLACEMENT EFFECT

By J. G. PRATT AND H. FORWALD

**ABSTRACT: A series of "scaled" PK placement tests in which Mr. Forwald was the main subject were carried out at the Parapsychology Laboratory of Duke University. They were similar to tests made previously in Sweden in which H.F. obtained significant results working alone with himself as subject. The primary aim of the new research was to try to confirm those earlier findings under conditions providing a second observer and independent recording of the results.**

**The procedure was very much like that used in the Swedish tests. For each trial, six wooden cubes were electrically released so that they rolled down an incline and spread over a horizontal surface marked off in centimeters. The subject "willed" them to go to the A- or B-side of the table according to which was the target area. Success was measured, not by the number of dice on the target side, but by the average distance the dice rolled in the specified direction. At each experimental session, five releases were made for the A-side followed by five for the B-side. The first throw for each side was of special interest since it was this part of the data that had consistently yielded extrachance results in H.F.'s previous work.**

**There were four exploratory sections of the research, involving a variety of conditions. On the basis of the results, a confirmatory section was undertaken in which it was predicted that H.F. would get statistically significant scores when he worked with one special assistant acting as co-subject and independent recorder. The prediction was realized: the total results on the first throw for each side in this section of the experiment were of an order that would be expected by chance only once in about 5000 such experiments. The results of this confirmatory series with double observation and recording of the data have the status of an independent confirmation of the earlier PK placement effects of H.F.—Ed.**

### INTRODUCTION

THIS PAPER is a report of PK placement tests carried out in the Parapsychology Laboratory of Duke University in October and November, 1957, during a visit by one of the authors, H.F. Over

a long period of time, working alone in Sweden with himself as subject, H.F. had obtained highly significant and remarkably consistent PK placement results. The purpose of his visit to Duke was to try to confirm his findings with the assurances against error provided by a second observer and by independent records of the results.

A word is needed at the very outset regarding the division of responsibility in this report. To H.F. belongs the major credit: his role as the principal subject was an indispensable one in the undertaking, and the success achieved is due in no small measure to his strong enthusiasm and unfailing energy. In spite of his status as an experimenter, he willingly submitted himself as a subject for this investigation of his PK ability; and this paper is an objective report of the results. J.G.P. had primary responsibility in the planning and supervision of the work and in the writing of this report.[1]

The work of H.F. is outstanding in the area of PK investigations. The first report of it was published by J. B. Rhine in 1951. Since then, H.F. himself has published seven experimental reports covering six years of uninterrupted research. (A complete list is given at the end of this paper.) No effort will be made here to summarize his earlier work in detail. However, it is necessary to characterize it in general and to call attention to a particular aspect of the data that has come to be the chief identifying characteristic of H.F.'s PK performance.

All of his work has been concerned with the use of PK to influence the placement of falling cubes. The objects have been released electrically at the top of an incline so that they fell under the force of gravity and spread out upon a horizontal throwing surface. This surface was divided by a center line into equal right-hand and left-hand areas, designated *A* and *B*. The two sides were used equally often as target area.

During the first three years, H.F. recorded his results only in terms of the number of cubes on each throw which came to rest inside the target area. His results were highly significant at the

[1] We wish to express our appreciation to the members of the Parapsychology Laboratory staff who helped to plan and carry through this research. We cannot list them all by name, but we wish especially to thank Dr. J. B. Rhine for his general counsel and support and Mrs. Peggy Murphy for her part in the experiments.

beginning of this period, but there was a marked decline in success thereafter. However, his work of this first period as a whole was still highly significant.

The general procedure was to throw five times for one target area, followed by five throws for the other. The evidence for PK was derived not only from the total scores but also from the *distribution* of scoring within the five successive releases made for one side as target. When the five-throw sequences were pooled, the highest rate of success was found on the first and last trials. This gave a U-curve such as had been reported previously for both the PK and ESP results of other subjects.

In 1954 H.F. published his first results using a new method of recording, the "scaled placement" test. In these tests he recorded to the nearest centimeter the lateral position in which each cube came to rest on the table. Success thus became measurable in terms of the degree to which the cubes were influenced to go toward the target side of the table. These data lent themselves to assessment by more efficient statistical tests of significance than did those of the previous period.

From the beginning of the scaled tests, it became apparent that the significance of the results was primarily due to the outstanding success of the first throw for the *A*-side as target, and to a marked degree also to the first throw for the *B*-side. Since the comparison of *both* targets is required as a control against any possible bias in the apparatus, this beginning salience in the five-throw sequence within the set was stated in terms of the *B-A* difference[2] in averages for Throw 1. The Throw 1 effect persisted in H.F.'s own placement work (but was generally not present on the few occasions when he used other people as subjects) right up to the time his research in Sweden was interrupted by his visit to the Laboratory. In his last published report he combined all of the Throw 1 results for his own scaled placement series. These included a total of 1452 cube readings, and the *B-A* difference gave a Student's *t* of 10.45, with P

[2] The centimeter lines were numbered from 0 on the *A*-side to 80 on the *B*-side, with the center line 40. Thus a *positive B-A* difference is in the desired direction. In all the preceding reports and throughout the present one the *B-A* difference has been obtained by getting the average position of the cubes when the *A*-side was target and the average when the *B*-side was target and then subtracting the former from the latter. Thus any given *B-A* difference is the average difference in placement per cube-pair.

less than $10^{-20}$. The *B-A* difference for the other four-fifths of the data was not significant.

The point emphasized here is that the *B-A* difference on Throw 1 had become a highly consistent and identifying mark of H.F.'s PK placement results in his published reports. This was true to the degree that H.F. had adopted the procedure of selecting the results of Throw 1 for separate statistical treatment and as the basis for his conclusions. During his Duke visit, therefore, the first important objective was to find out whether the Throw 1 effect would hold up in his results. If it did, then further tests would be designed with the plan of selecting those data for separate evaluation.

H.F. advanced his work in Sweden beyond the point of merely gathering more evidence for PK. He has for some time been concerned with questions related to the energetics of the PK placement process. However, because of the limited time that he was able to spend at the Laboratory, it was not possible to go further into this aspect of his research. Rather, it seemed advisable to concentrate upon a single objective, that of confirming his basic PK effect.

## Apparatus

A PK placement test apparatus was constructed by H.F. after his arrival at the Laboratory. Since this is essentially a duplication of the set-up which was used in Sweden, a general description will suffice in the present report.

A release mechanism is mounted above an incline which curves downward and joins one end of a horizontal throwing surface. Before being released, the cubes lie in a row starting at the back end of a wooden V-shaped channel which is held in a horizontal position. The cubes are released by pressing a control button attached to an electro-magnetic release mechanism at the end of a long, slack cable, part of which lies on the floor. When the button is pressed, the end of the V-channel toward the incline drops and the cubes fall and spread out over the horizontal surface. A low wooden wall on the three sides of the table away from the incline prevents any of the cubes from falling off onto the floor.

The surface of the table is made of thin, transparent plastic stretched over a layer of plate glass. Under the glass there is a sheet of lined co-ordinate paper with numbered centimeter intervals.

The longitudinal lines are numbered from 0 at the right-hand (*A*) edge of the table to 100 at the left-hand (*B*) edge. This is the scale by which the lateral position of the cubes is recorded. It will be noted that the Duke table is 20 cm. wider than the one used in Sweden.

The cross lines are numbered from 0 at the foot of the incline to 120 at the far end of the table. This scale is used to record the distance each cube rolls along the table before coming to rest. While observations of longitudinal distance were made in the Duke work, these data were not required in the analyses covered by the present report and they will not be referred to again in this paper.

The cubes in the Duke experiment measured 5/8 of an inch on each edge and were made of seasoned beechwood. They had sharp corners and smooth surfaces, and the same six cubes were used throughout the experiment. In the first series, D1, the surfaces were covered with squares of silver, 48 mm. thick, but in all the other series they were uncovered.

## Procedure

Two main aspects of the experimental procedure need to be distinguished. The first is the general strategy by which the research was guided toward the objective of getting a confirmation of H.F.'s PK effects. The second is the tactical one: the details of how the tests were conducted.

### *General Plan of the Experiment*

For H.F., shifting the scene of his operations from the basement of his own home, where he had worked previously, to the Parapsychology Laboratory involved a big change in the psychological conditions. It was necessary to try to minimize the effects of this shift. The best approach seemed to be to proceed cautiously, allowing the plan to develop from the results themselves. To reduce the risk that H.F. might become self-conscious and inhibited in the new situation, he was not invited to share in the discussions regarding the general experimental approach. H.F. co-operated wholeheartedly under this limitation, even though he was sometimes asked to take part in a test which he would not himself have chosen to do at that particular time.

Since it was not possible to say in advance what the effect on the results would be of having a second person present to observe and record them, it was necessary to carry out exploratory tests to find the right conditions for success. Only after that had been accomplished would it be possible to set up a confirmatory experiment. It did not matter how much time and how many trials went into the exploratory stage of the work. What did matter was that this stage should provide a clear-cut basis of prediction regarding the effects anticipated in the confirmatory series. Only statistically significant *predicted* results in the confirmatory tests could fully meet the objectives of the research.

One consequence of not making H.F. a full partner in deciding upon the general strategy of the experiments was that he sometimes wished to introduce variations in conditions that were not essential to the main purpose of the investigation. He was encouraged to follow his own inclinations as long as these additional exploratory tests did not interfere with the tests directed toward confirmation of his PK abilities. The shortness of the visit put H.F. under pressure to work at a faster pace than in Sweden. Some of the test variations which H.F. himself wanted to introduce seemed to ease this pressure somewhat. These were series in which someone else was designated as the main subject in the tests and pressed the release button for each throw of the cubes. While H.F. himself was present during these tests and therefore might have contributed to any PK results obtained, his main function was to be the observer and recorder.

The tests are divided into four exploratory sections, A through D, and one confirmatory section, E. The sections were introduced in the research in alphabetical order, but the tests of one section were not always completed before the next was begun.

*Section A.* H.F. worked alone, as he had done in Sweden. The object was to see if he could duplicate his earlier success under conditions similar to those he had used previously.

*Section B.* H.F. was the subject, and a laboratory staff member was present as a passive observer and independent recorder of the results.

*Section C.* H.F. continued to act as the principal subject and to release the dice, and a staff member was once more present as an observer and independent recorder. This staff member, however, had an additional part in the experiment: he consciously attempted to help H.F. influence the dice.

*Section D.* Someone other than H.F. was the principal subject and releaser. H.F., and a third person when there was one, also consciously attempted to help the subject. H.F. acted as the only recorder, witnessed by the other one or two persons present.

*Section E.* The confirmatory section comprised tests made with H.F. as subject and P.M., the one observer with whom H.F. had worked successfully in Section C, as co-subject. Both recorded independently.

*Detailed Test Procedure*

Before each release of the six cubes, the release mechanism was reset by lifting it with one finger until it was automatically caught and held by the anchor of the release magnet. The cubes were swept together on the table and, held in a row by the pressure of two fingers, were lifted and placed so that the back cube was even with the back end of the V-channel. This left approximately two inches of clear space at the front end of the channel. Care was taken to see that all of the cubes were resting securely on the bottom of the channel so that they were touching on both sides. The resetting of the release mechanism and the placing of the cubes was always done by H.F. and was observed by others present.

Each person taking part in a test chose his own position, but no one was in contact with the apparatus or the table on which it rested when the cubes were released. H.F. stood on the right-hand or *A*-side of the table, as he was in the habit of doing from his earlier work. The other person or persons generally stood on the opposite side. No change of position occurred when the shift of target was made from the *A*- to the *B*-side.

The cubes were released by pressing the control button. As soon as the cubes had come to rest, their positions were observed and recorded. This was always done by H.F., and in some sections of the tests, as already explained, by a second person as well. In making

the two records, each observer worked alone and completed his recording without taking any notice of the other. When both were finished, the two records were compared; and in case any discrepancy was found, each observer rechecked the cube or cubes involved, and agreement was reached as to which of the two records was accurate. The infrequency of discrepancies showed that H.F. had higher than 99% accuracy in his recording; and as the second observer became more experienced at the task, he soon approached the same level. In almost every instance when an error occurred, it was only when the cubes were difficult to judge for two adjacent centimeter lines, and not as a result of mistakes in reading the centimeter scale. In those few cases in which the cubes were so nearly balanced between two lines as to make a judgment impossible, the results were recorded so as to disfavor the target side.

The trials were made in *sets* of 10 throws, 5 for the *A*-side as target, followed by 5 for the *B*-side. Once a set was begun it was finished without interruption, and generally only one set was done at one experimental session. Five sets under a given experimental condition constituted a *series*. Since six cubes were released at a time, a set thus consisted of 60 trials; and a series, of 300 trials.

## Results

A total of 14 series were conducted but only the last two of them came within Section E, the confirmatory part of the research. However, since there was a logical development in the work before the confirmatory tests were reached, it is necessary to trace the progress of the research through the different exploratory sections. This serves two purposes: it summarizes all of the work done, and it gives the results which made possible a prediction for the confirmatory stage. The second purpose is the more important one for the present report.

### *Exploratory Results*

Section A comprises two series in which H.F. worked entirely alone; he was both subject and recorder. There were two series in this section. In Series D1 the incline was a hard composition board with the smooth side up. During this series H.F. got the impression that the smoothness of the incline did not produce sufficient tumbling

action of the cubes. Before starting Series D2, therefore, the incline was covered with a cloth of coarse weave, and this remained in use throughout the 13 remaining series.

Table 1, which summarizes the results of the research, series by series, shows that Series D1 and D2 gave statistically insignificant total results. However, they both gave a significant *B-A* difference for Throw 1. Section A therefore represents a successful repetition of H.F.'s main PK placement effect, the outstanding Throw 1 difference, when the experimental conditions in the Duke Parapsychology Laboratory were like those used in Sweden.

On the strength of the Section A results, an important advance was tried in the next exploratory stage (Section B): a member of the laboratory staff came in as an observer and independent recorder. During two series, D3 and D4, the observer (P.M.) made herself as inconspicuous as possible during the tests, for it was recognized that the introduction of a second person might change the psychological conditions and thus affect the results. The hope was, however, that any upsetting effect might be minimized or avoided by having the observer be as unobtrusive as possible. P.M. therefore sat in a corner of the room while the dice were released and came forward only to make her record while H.F. was making his, and she remained near the table only long enough to compare and confirm the two records for the throw.

As may be seen in Section B of Table 1, however, the observer nevertheless seemed to upset the scores. There were no significant differences between *B-A* averages in either series, either on Throw 1 or in the data as a whole. Obviously some new exploratory approach was called for.

During the period covered by Section B, H.F. commented on several occasions that the presence of the second person in the room distracted his attention, and at the end of the Series D4 he himself suggested a change in the procedure which might overcome this difficulty. The proposal was that the observer should also act as subject. This was tried in Section C, which was made up of three series, in each of which a different member of the laboratory staff tried to help H.F. influence the cubes. This member also made an independent record of the results. In Series D5, P.M., who had been the passive observer in Section B, was the active co-subject,

and significant results on Throw 1 were secured. The *B-A*-difference in averages was +15.47, which yielded a $t_d$ of 2.75 (58 d.f.), with a P = .006. The data for all throws combined were also significant, with a P of .01. It is clear, however, that this result is due to the big difference on Throw 1. In Series D8 and D12, on the other hand, in which different staff members took the place of P.M. as co-subject and recorder, the results were at the chance level. H.F. commented that in the presence of these people, even though each was trying to act as a subject, he became self-conscious and distracted in his own efforts to influence the cubes. But since he thought that P.M. entered more fully into the spirit of the test when she was co-subject, he felt that he was able to concentrate upon the task to the same degree as when working alone. On this basis, therefore, Section E, with P.M. as co-subject and independent recorder, was set up to see if the results of Series D5 could be confirmed.

The decision to conduct further confirmatory series with H.F. as the main subject and P.M. as co-subject (D13 and D14) was reached in a laboratory staff discussion of the results in which attention was directed especially to the success of this team. In the same discussion the decision was made to do further exploratory tests in which H.F. had other co-subjects besides P.M. to see if another successful combination could be found. In the time available, only one other such exploratory series was completed (D12), and this gave only chance results.

Meanwhile, just after the tests of Section C were begun, H.F. started several series, Section D, in which he had someone other than himself take the role of the main subject and release the cubes. These tests must be classified as exploratory since no independent record was made and since the subjects were all untried and therefore unselected. Work which Forwald had done previously in Sweden with persons other than himself had not shown the outstanding Throw 1 difference, and thus there was no basis for predicting this pattern of success in Section D. Only one out of five series completed in this section of the experiment gave a significant Throw 1 difference. This was Series D10, where the Throw 1 difference in averages was +12.23, with $t_d = 2.38$, and this result (P = .017) is not strongly suggestive when an appropriate correction for selec-

tion is made. There was a consistent and slightly positive *B-A* difference in the data of these five series as a whole, but no significant results.

*Confirmatory Results*

The confirmatory tests consist of the two series of Section E. The first set of Series D13 was done on November 12 and the last set of D14 on November 21. Thus the work was carried out at the

Table 1

TOTAL RESULTS

| Section* | Series | Subjects | Throw 1 | | | | All Throws | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | B−A diff. | $SD_d$ | $t_d$ | P | B−A diff. | $SD_d$ | $t_d$ | P |
| A. (Exploratory) | D 1 | H. F. alone | + 7.67 | 2.85 | 2.69 | .0071 | +2.36 | 1.35 | 1.75 | .080 |
| | D 2 | H. F. alone | + 9.63 | 4.04 | 2.38 | .017 | +1.25 | 1.89 | .66 | .51 |
| B. (Exploratory) | D 3 | H. F. | + 2.73 | 5.17 | .53 | .60 | − .51 | 2.40 | .21 | .83 |
| | D 4 | H. F. | − 3.43 | 4.86 | .71 | .48 | + .75 | 2.48 | .30 | .76 |
| C. (Exploratory) | D 5 | H. F. & P. M. | +15.47 | 5.62 | 2.75 | .006 | +6.53 | 2.53 | 2.58 | .010 |
| | D 8 | H. F. & J. G. P. | − 2.00 | 4.62 | .43 | .67 | −1.99 | 2.19 | .91 | .36 |
| | D 12 | H. F. & F. G. | − 4.13 | 5.22 | .79 | .43 | − .27 | 4.42 | .06 | .95 |
| D. (Exploratory) | D 6 | L. L., A. F., & J. F. | − 2.53 | 5.05 | .50 | .62 | +4.43 | 2.14 | 2.07 | .038 |
| | D 7 | J. F. & H. F. | + .63 | 4.69 | .13 | .90 | +4.18 | 2.13 | 1.96 | .050 |
| | D 9 | M. J., H. F., & W. S. | + 9.87 | 5.70 | 1.73 | .084 | +2.09 | 2.29 | .91 | .36 |
| | D 10 | C. B., H. F., & J. F. | +12.23 | 5.13 | 2.38 | .017 | +3.49 | 2.27 | 1.54 | .12 |
| | D 11 | A. L., H. F., & J. F | + 5.17 | 4.99 | 1.04 | .30 | +3.81 | 2.17 | 1.76 | .078 |
| E. (Confirmatory) | D 13 | H. F. & P. M. | +21.47 | 5.66 | 3.79 | †{ .00015 | +4.01 | 2.58 | 1.55 | .12 |
| | D 14 | H. F. & P. M. | + 8.80 | 5.25 | 1.68 | .093 | +3.91 | 2.32 | 1.69 | .091 |

†Combination of P-values by Fisher's method
$\chi^2 = 22.36$ (4 d.f.)
P = .0002

*Conditions for different sections:
A: H. F. working alone in dual role of experimenter and subject.
B: H. F. acting as subject and recorder; P. M., as passive observer and second recorder.
C: H. F. as main subject; a staff member as a co-subject; both recording independently.
D: Someone brought in as a new subject for each series; H. F. present only as co-subject and he alone recorded.
E: H. F. as main subject; P. M. as co-subject; both recording independently. This section was set up as a confirmation of Series D 5.

average rate of more than one set per day—faster than the usual rate for H.F. in Sweden. The experiment was terminated after Series D14 because H.F. had to return to Sweden at that time.

As previously decided, Throw 1 was again selected for separate evaluation. In Series D13, the *B-A* difference on Throw 1 was +21.47. This is the largest difference obtained in the work at Duke University and it was not equalled by any of H.F.'s previous results. The $t_d$ is 3.79, with P = .00015. In Series D14 the *B-A* difference in Throw 1 was +8.80, for which $t_d = 1.68$ and P = .093. When the two probabilities are combined by Fisher's method, the result is: $\chi^2 = 22.36$ (4 d.f.); P = .0002. The results (shown in the last section of Table 1) confirmed the prediction made on the basis of Series D5, and thus they constitute a general confirmation of H.F.'s PK placement effect under laboratory conditions safeguarded by independent observation and duplicate recording of the results.

As was expected from H.F.'s earlier results, there was no appreciable *B-A* difference in the data other than that found on Throw 1. Section E as a whole does not yield a statistically significant difference even when the Throw 1 results are included.

## Discussion

The manner in which the tests developed makes it necessary to focus attention upon the *B-A* difference of Throw 1 in Series D13 and D14. The level of significance found there clearly indicates an effect in the placement of the cubes which cannot reasonably be attributed to chance. What about other counterhypotheses to the PK interpretation?

The presence of a second observer and the making of an independent record of the results rule out recording errors as a possible explanation of the results. The Throw 1 differences in averages in the crucial series are of an order of magnitude expressed in several centimeters. Difficulty in reading the position of the cubes was found only for those balanced on one centimeter line or between two lines; and this area of uncertainty never represented more than two millimeters. Under these circumstances, independent records provide adequate assurance against recording errors as a relevant factor in the results.

P.M., the laboratory staff member present during Series D13

and D14, was fully aware of the importance of clearly deciding and recording in advance which side of the table was to be the target at the beginning of a new series. She was aware also of the importance of counting every throw, and she knew that the purpose of her presence was to exclude explicitly the counterhypothesis that the pronounced Throw 1 effect might arise from a practice of making off-the-record throws without a sufficiently clearcut indication of when the next throw was to be counted. H.F. was questioned regarding the possible bearing of this counterhypothesis upon his results and he stated that it was not his practice to make off-the-record throws. When he intended to carry through an experimental set, the record was started with the very first release of the cubes. P.M., as well as other observers who worked with H.F. during his visit, were able to support this statement for the trials which they observed.

Is it conceivable that there was some bias in the apparatus which produced the Throw 1 effect? This would have had to be a systematically shifting bias. The sturdy construction of the apparatus and the very slight effort required in resetting the release mechanism make it seem most unlikely that any effect of the sort required to produce the result could have been brought about either consciously or unconsciously. The cubes were handled in a uniform manner in placing them in the release mechanism. They almost always settled snugly into the release channel under the force of gravity. These observations argue compellingly against the possibility that the manner in which the cubes were handled could have caused the results. If additional argument on this point is needed, it can be found in the fact that there were no significant results in other parts of the investigations when H.F. gave every sign of very much wanting to succeed. In Section B, for example, he was very highly motivated to get results since he had just been successful alone (Section A) and for the first time he was under pressure to produce evidence with an observer present. His lack of success there is reasonably explained on *psychological* grounds, but not on the grounds that a normally faulty apparatus suddenly lost its bias—only to regain it as a later stage. Furthermore, it is difficult to explain why the results should be restricted to Throw 1 if there was any sort of bias in the apparatus. As far as it could be observed, the releasing was done in the same manner throughout the set; and if there was any bias favoring

the *A*-side on the first throw for that target, it should have favored the *A*-side for the other four throws as well, and similarly for the five throws for the *B*-target.

The only counterhypothesis remaining would be one involving a conscious or unconscious collusion between H.F. and P.M. When a single investigation has progressed to the point of making it necessary to introduce this possibility as the only alternative to the PK hypothesis, it has gone as far as it is possible for one piece of research to go to offer evidence for PK. The conclusion, therefore, is that the Duke investigations have confirmed the Forwald PK placement effect.

Thus far attention has been focused only upon the aspects of the results which were clearly selected in advance as the basis for evaluation and judgment regarding confirmation. Now that a conclusion has been reached on that primary issue, however, it is interesting to re-examine the data from a more free-ranging, exploratory point of view for suggestions bearing on the PK effect. For this purpose the data of the exploratory sections of the experiment may be considered along with the results of the confirmatory stage, since the purpose is not to reach final conclusions, but to gain insights which might help in further investigations and might contribute toward a better understanding of the PK process.

The fact that significant *B*-*A* differences have been found so often in Throw 1 but not in the other throws of the set is another way of saying that the data showed "beginning salience" or a decline effect. This suggests that the results might be examined more closely for scoring trends within the five-throw unit. H.F. had already done this kind of analysis in some of his earlier reports, and he looked for position effects in the Duke results as the work progressed. He divided the data in which he was the "main" subject into two groups: those series in which the scoring was outstandingly successful on Throw 1 (D1, D2, D5, D13, D14); and those in which his usual success on Throw 1 was not achieved (D3, D4, D8, D12). This left the group of five series in which someone else was the "main" subject and in which the results, while generally positive, showed large series-to-series variations (D6, D7, D9, D10, D11). For whatever suggestive value they might have, these five series

were combined in a third group for the study of the distribution of scoring within the set.

The distribution of *B-A* differences for the five positions of the set were plotted for these three groups of data. The two positive-scoring groups were similar in distribution: they both gave roughly U-shaped curves. The low-scoring series with H.F. as the principal subject gave an inverted curve of the same shape (See Fig. 1). Thus there is a strong suggestion that opposite scoring trends may have been present in the different tests according to whether the psychological conditions were favorable to PK success or unfavorable.

Further breakdowns may be made within the groups upon which the curves of Fig. 1 are based, particularly within two groups

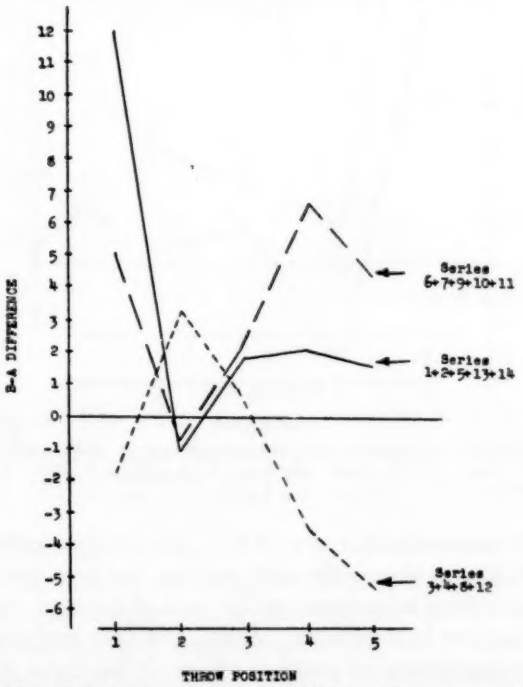


FIG. 1. Position effects in terms of the *B-A* difference in average within the five-throw sequence of the set.
Series 1 + 2 + 5 + 13 + 14: High-scoring series with H.F. as main subject.
Series 6 + 7 + 9 + 10 + 11: High-scoring series, but H.F. not main subject.
Series 3 + 4 + 8 + 12: Low-scoring series with H.F. as main subject.

with H.F. as the main subject. Fig. 2 shows the curve for his positive-scoring series separated into the subgroup in which he worked alone (Series 1 and 2) and the subgroup in which he had P.M. as co-subject (Series 5, 13, and 14).

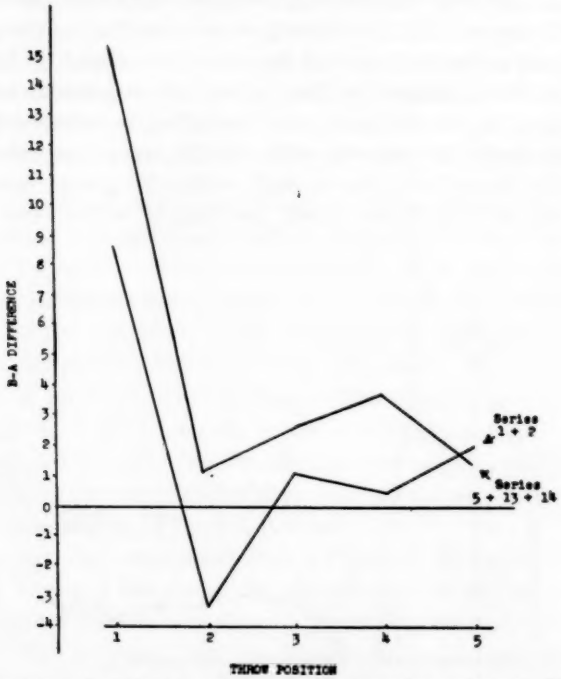


FIG. 2. Position effects in the pooled sets of high-scoring series with H.F. as the main subject when entirely alone (Series 1 + 2) and with P.M. as co-subject (Series (5 + 13 + 14).

In a similar manner, the curve of Fig. 1 for the low-scoring group, in which H.F. was always the main subject, has been broken down in Fig. 3 into three subgroups, one for each of the three people who acted as observers in these tests. Again, this finer analysis shows a remarkable consistency of position effects in the three subgroups. The two curves of Fig. 3 comparing the results when P.M. was the passive observer and when J.G.P. was attempting to act as co-subject are so close as to be almost identical, and a regression test of the

degree of similarity yields a significant result: Student's $t = 8.9$ (3 d.f.); $P = .001$. The distribution for H.F. working with F.G., the second person with whom he failed when she was co-subject, is also quite similar to these two, as may be seen by inspection of Fig. 3.

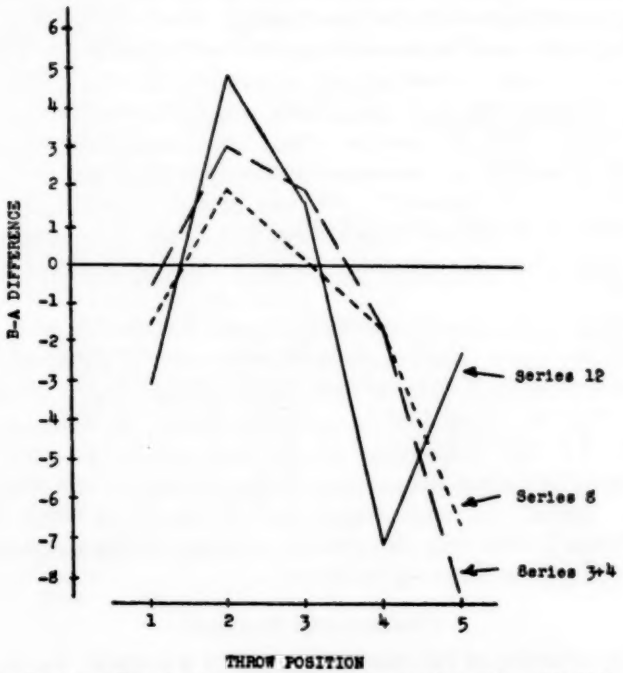


FIG. 3. Position effects in the pooled sets of low-scoring series with H.F. as the main subject.
Series 3 + 4: P.M. present as passive observer and recorder.
Series 8: J.G.P. present as co-subject and recorder.
Series 12: F.G. present as co-subject and recorder.

Since the two curves evaluated by the regression test are obviously the two (out of three) which are most closely similar and since there are three such comparisons possible, an appropriate correction for selection within Fig. 3 would be to multiply the P value by a factor of 3, giving a corrected P of .003, and further correction would be required if selection among all three figures were to be considered.

However, the quantitative evaluations of these curve comparisons

need not be emphasized, because the position effects were not sought as the primary evidence bearing upon whether or not the PK placement effects would be confirmed, and there is no need to present them in this light. Nevertheless, they lend some support to the evidence for PK obtained in Section E, the confirmatory stage of the research. It is interesting to find the same scoring trends in H.F.'s positive scoring series when he was working alone and when he was working successfully with a co-subject and independent recorder. This similarity suggests that H.F. was chiefly responsible for the results in both cases. Also, the position effects found among the three subgroups when H.F. did not score positively suggest that even these results were not "chance." PK was possibly operating in these tests as well, distributed in such a way that it was obscured when the data were considered either for Throw 1 alone or for the sets as a whole.

These studies of position effects suggest that the Throw 1 effect is only one aspect (usually, the outstanding one) of a general, overall structuring by H.F. of the task represented by the set. However, even when the Throw 1 effect is not pronounced, as in the series in which H.F. was unsuccessful as the main subject in attaining a significant deviation, structuring of the five-throw unit may still occur. Indeed, the results suggest that the conditions which inhibit the Throw 1 effect may also result in a reversal of the scoring trend in the other four throwing positions.

## Concluding Remarks

The objective of the research covered by this report was easy to define. No one anticipated, however, that it would be easy to achieve. The task facing H.F. in coming into a strange situation for the sole purpose of demonstrating his PK abilities under rigorously controlled experimental conditions was one which presented many obstacles. That the task was carried through with such success must be credited to H.F.'s confidence in his PK abilities and his capacity for concentrating upon the tests under difficult conditions.

Confirmation of H.F.'s earlier results was not expected greatly to advance scientific knowledge about how PK operates. It was intended, rather, to put his research on the more secure basis afforded by independent observation and recording.

The present investigation has not been directly concerned with the recent studies of H.F. dealing with the energetics of PK placement. But there can be no doubt regarding the importance of the problems with which that more advanced PK research has been concerned, and it is perhaps justifiable to hope that other investigators will soon be doing productive research in the PK placement area.

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# ESP SCORE LEVEL IN RELATION TO STUDENTS' ATTITUDE TOWARD TEACHER-AGENTS ACTING SIMULTANEOUSLY

By MARGARET ANDERSON AND RHEA WHITE[1]

**ABSTRACT: Three previous experiments carried out in the classroom with the students as subjects have given evidence that the attitudes of teachers and students toward each other affect the ESP scoring of the subjects. Since these researches were clairvoyance experiments, the teacher did not act as "sender" though he did present the test to the class. The research reported here was intended to explore the question of whether the student attitudes would affect ESP scoring when teachers were used as senders.**

**Two teachers (X and Y) and 51 high-school students took part in the experiment. The two teachers sat in a room separated from the subjects and looked at ESP cards, one by one, while the students tried to guess what the "target" cards were. The two target decks were not alike, but this fact was not mentioned to the subjects. After the tests, the students were asked (1) to name which one of all their teachers they liked best; and (2) to name three others whom they would prefer as teachers.**

**The results showed that when teacher X was on the "best liked" or "preferred" list, the students' ESP scores were above chance, and when this teacher was not on either list, the scores were below chance. This relationship between attitude and ESP scoring for teacher X was significant. For teacher Y, it was not, though the relationship did hold true to a significant degree when the results with both teachers were combined.**

**The authors point out that the over-all scoring averages show the same general trends as those of the previous experiments and that it might be worth while to make a special study of the role of the individual teacher in such ESP tests.—Ed.**

## INTRODUCTION

THE SCHOOLROOM has in recent years been found to be a living laboratory for the study of factors that influence psi performance. The experiments of Van Busschbach (**6**, **7**, **8**) with fifth- and sixth-grade children in Holland and the United States strongly suggest that ESP occurs in the classroom when the teacher is the agent in a GESP test; and clairvoyance tests carried out by the authors (**1**, **2**, **3**) indicate that the interacting attitude between teacher and student influences the direction of ESP scoring (that is, it is associated with above- or below-chance averages).

[1] The authors wish to thank the high-school principal, the two teachers, and the 51 students who made this experiment possible. Both authors were supported in this work by Ralph Drake Perry Fellowships.

The research reported here was an exploratory effort to ascertain if a student's attitude toward a teacher would influence his scoring in a GESP test when two teachers, acting as agents, simultaneously looked at different random orders of targets.

The present experiment is similar in general design to work previously reported by MacFarland (**4**) and by Soal and Bateman (**5**), in both of which the subject on each trial made a *single* call for the targets of *two* agents. Both of those studies showed a significant rate of scoring with one agent but chance results with the other. The object of the present experiment was to find out whether discrimination between teacher-agents in such a dual-target ESP test is related to the attitudes of the student-subjects toward the teachers. The experiment as planned thus combined elements from several earlier investigations: the *dual-target* experiments such as the two mentioned, the work of Van Busschbach involving *GESP* tests in the public schools, and that of the present authors who studied ESP and attitudes in *clairvoyance* tests in the classroom.

From the outset the present research was regarded as exploratory because it involved new conditions and thus lacked a firm basis for predicting the outcome. One change, for example, was the use of two teachers simultaneously, which represented a departure from the regular classroom situation. These changes were seen as enhancing the suggestive value of any similarity which might turn up between the findings of this experiment and those of the earlier schoolroom experiments. It was hoped that the results would provide additional clues to the nature of the attitude-ESP relationship and point the way to further experiments.

## Procedure

Two large ESP packs consisting of 125 cards each were made up from *Tables of Random Sampling Numbers* by Kendall and Smith. An original as well as a carbon copy of each target order was made, then double-checked against the card orders, and put on file in the Parapsychology Laboratory at Duke University.

The experiment was a group GESP test in which the 51 members of the senior class of a small high school were the subjects and two teachers were the agents. Every subject had been taught by each teacher-agent in at least one class.

The test was conducted in the high-school auditorium during April, 1956. The students were called together by the school principal. They had not been told about the forthcoming test, but at the request of the authors, the principal had previously discussed the project with the two teachers. During the experiment both teacher-agents were situated in a dressing-room to the right and off stage at the front of the auditorium. One of the authors, M.A., directed the students in the auditorium, while the second author, R.W., remained in the dressing-room to direct the teachers.

M.A. passed out ESP record sheets to the students and familiarized them with the five ESP symbols which were to be the targets. The students were instructed to write their names at the tops of the sheets and later to fill in the first five call columns, making a total of 125 guesses. They were told that two of their teachers, X and Y, were in the dressing-room and that these teachers would be looking at 125 ESP cards, one at a time. The students were also told the names of the two teachers. At a signal from R.W. (a tap on the dressing-room wall), they were to write down the symbol at which they thought the teachers were looking. It was purposely not made clear whether the teachers were looking at the same card or at different ones. Instead, the pupils were merely told to write down what they thought was the "correct" guess for Call 1, Call 2, etc.

The two teachers sat at separate desks in the dressing-room, ten feet apart and back-to-back. Having made certain that the teachers were looking at the first card of their respective packs, R.W., who could not see the cards, knocked on the wall with a heavy object, and, at this, each student wrote down his first guess. After five seconds, R.W. signaled again, the teachers looked at the second card, and the students recorded their calls for this card. This was continued until the 125 guesses were completed.

After the ESP test was finished and while R.W. was conversing with the two teachers (still in the dressing-room), M.A. asked the students to turn over their record sheets and answer the following two questions:

1. Write the name of the teacher you have or have had in high school whom you like the best.

2. Name three others you would prefer as teachers. (Do not list a teacher whom you do not like.)

The score sheets were then collected.

Using the original master sheet of the targets, as well as the carbon copy, the two authors independently checked the data here presented. In addition, a third and final check was made by Mrs. Betsy Moser of the Parapsychology Laboratory.

## Results

The 51 subjects completed a total of 255 standard ESP runs, or 6375 trials. The results, in accord with the experimental design, were analyzed in relation to the student's attitude toward the teachers as reflected by the questionnaire.

The extent to which ESP score averages are related to the student's feeling toward the teacher is shown in Table 1. Seven students, who in answer to Question 1 named the teacher X as their best teacher, give an average run score of 5.43 on her targets. These same seven subjects give an average run score of 4.17 on the targets looked at by teacher Y. Similarly, seven students naming Y as their best teacher give an average run score of 5.09 on the targets looked at by Y and 4.77 on the target pack of X.

Table 1

ESP Results as Related to Student's Attitude Toward Teachers

| Questionnaire Category | No. of Subjects | No. of Runs | Av. Run Score With Teacher X | Av. Run Score With Teacher Y |
|---|---|---|---|---|
| (1) Teacher X Best | 7 | 35 | 5.43 | 4.17 |
| (2) Teacher Y Best | 7 | 35 | 4.77 | 5.09 |
| (3) Teacher X Preferred | 13 | 65 | 5.17 | 5.03 |
| (4) Teacher Y Preferred | 10 | 50 | 4.78 | 4.80 |
| (5) Both Teachers Preferred | 9 | 45 | 5.31 | 4.78 |
| (6) Neither Teacher Preferred | 5 | 25 | 4.24 | 4.80 |

Thirteen other students who placed teacher X (but not Y) in the second category as to preference give an average run score of 5.17 on the targets looked at by X, as contrasted with an average of 5.03 on the target pack of Y. The average run score for the 10 students who put Y (but not X) in the second category is 4.80 for Y as compared to 4.78 for X.

Nine students indicating a preference for both teachers over others give an average run score of 5.31 with teacher X and 4.78 with teacher Y.

For the five students who had no preference for either teacher, the average run score is 4.24 with teacher X and 4.80 with teacher Y.

The above results, which are summarized in Table 1, are given for purposes of general comparison only. They are not evaluated by the CR method, usually employed, for two reasons: first, the use of only two target orders for the 51 subjects introduces the possibility of a "stacking" effect, but because of the exploratory character of the research it was not deemed worth while to apply the laborious method required for a proper statistical evaluation of these results; second (and more important), the question with which the paper is concerned—that of the relation between ESP and student attitudes toward the teacher-agents—can be judged only in terms of the consistency of the subjects' ESP scores and questionnaire answers. In other words, not the individual ESP trials (as shown in Table 1) but the individual subject's score with each agent must be taken as the statistical unit of observation.

In order to evaluate the consistency of scoring on the basis of the students' answers to Questions 1 and 2, the results were arranged in two-by-two contingency tables. The following tabulation was carried out for the results with each agent separately and for both agents combined to give the distributions shown in Table 2.

The number of subjects whose total ESP score with a given agent was above M.C.E. (mean chance expectation) and the number of subjects whose scores were at or below M.C.E. were tabulated according to whether each student rated the teacher as either best or preferred, or did not list that teacher under either question. It should be pointed out that six of the 14 students who named either teacher X or teacher Y as best (Question 1) also indicated a preference for the other teacher (Question 2). However, in Table 2 each student is only counted once for each teacher. Thus in terms of the numbered rows of Table 1, the "favorable attitude" results are those in rows 1, 3, and 5 for teacher X and rows 2, 4, and 5 for teacher Y, while the "unfavorable attitude" results are those in rows 2, 4, and 6 for teacher X and rows 1, 3, and 6 for teacher Y. The results indicate that the relation between ESP and attitude was

Table 2

ESP Score Level in Relation to Student's Attitude Toward Teacher-Agent

| | Teacher X Best or Preferred | Teacher X Not Preferred | |
|---|---|---|---|
| Students with ESP Scores Above M. C. E. | 19 | 6 | 25 |
| Students with ESP Scores at or Below M.C.E. | 10 | 16 | 26 |
| | 29 | 22 | 51 |

$\chi^2 = 7.32$ (1 d.f.)
P = .007

| | Teacher Y Best or Preferred | Teacher Y Not Preferred | |
|---|---|---|---|
| Students with ESP Scores Above M. C. E. | 10 | 7 | 17 |
| Students with ESP Scores at or Below M.C.E. | 16 | 18 | 34 |
| | 26 | 25 | 51 |

$\chi^2 = .63$ (1 d.f.)
P = .44

Pooled Table

| | Teacher Best or Preferred | Teacher Not Preferred | |
|---|---|---|---|
| Students with ESP Scores Above M.C.E. | 29 | 13 | 42 |
| Students with ESP Scores at or Below M.C.E. | 26 | 34 | 60 |
| | 55 | 47 | 102 |

$\chi^2 = 6.57$ (1 d.f.)
P = .01

significant for teacher X ($\chi^2 = 7.32$, 1 d.f.; P = .007) but not for teacher Y. When the work with both teachers is combined in a two-by-two table,[2] the results are still significant ($\chi^2 = 6.57$, 1 d.f.; P = .01).

[2] Because of the fact that the figures in Table 2 involved ESP scores from a number of subjects calling the same target orders, it is not possible to state that they are statistically independent, and this requirement for a chi-square test is therefore not met. But since this is only an exploratory paper, the figures are presented, nevertheless, for their suggestive value. Naturally, any confirmatory study which may later be undertaken would need to avoid the multiple-calling difficulty either in the design of the experiment or by an appropriate method of statistical evaluation.

## Discussion

The main reason for presenting this research is to point out suggestive similarities between it and the school work of Van Busschbach and of A-W previously reported. In Holland and America, Van Busschbach obtained significant results in GESP tests in the fifth and sixth grades when the teacher acted as agent with his own class. A-W were predominantly interested in the relation between the level of ESP scoring and the attitudes of the students and teachers toward each other, especially when these feelings were *mutual.* The A-W tests were clairvoyance experiments; but the teacher, even though not acting as a "sender" or agent, did present the test to his class. The significant results showed that, with the teacher in that role, attitudes were important.

Both the Van Busschbach and the A-W experiments were conducted in the regular classroom. This report concerns another aspect of the same general area of investigation: Would student attitudes toward a teacher, presumably established during classroom contact, not only continue to show a relationship with the level of ESP scoring out of the classroom situation, but also show discrimination in favor of a preferred teacher in a two-agent GESP test?

The student questionnaire used in the previous A-W clairvoyance series was designed to determine the student's attitude toward the particular teacher in whose class the test was being conducted. In the present report, since the test was not confined to a classroom situation, the questions were worded differently in order to enable the student to register his attitude toward each of these two particular teachers in relation to all of his high-school teachers.

A comparison of this experiment with the three A-W series already reported further emphasizes the importance of student attitude. In those experiments, three levels—highly positive, moderately positive, and unfavorable attitudes—are directly related to the rate of ESP scoring. Students in the highest attitudinal category gave an average run score of 5.26 on 1103 runs. In a like manner, the students in the present experiment who listed the teacher as their first choice give an average run score on 70 runs of 5.26 (5.43 with teacher X and 5.09 with teacher Y). The moderately positive group in the A-W clairvoyance series scored an average of

5.17 on 800 runs as compared with an average of 5.02 on 205 runs in the present study (combining rows 3 and 4 with 5 in Table 1 to obtain the averages of 5.23 with X and 4.78 with Y). In the A-W series, students who indicated an unfavorable attitude toward the teacher gave an average run score of 4.64 on 708 runs. In the present experiment, the five students who did not list either teacher made an average score in 50 runs of 4.52 (4.24 with X and 4.80 with Y). This is not to say that these students necessarily were negative in their attitude toward the teacher; only that they did not list the teacher as one they liked best or whom they would prefer to have for a class.

The present study brings out another aspect of the relation between attitude and ESP scoring. In this experiment the students who listed one of the two teachers as "liked best" scored positively on the targets of that agent and negatively on those of the other even though they did not know that the two teacher-agents were looking at different cards. At the other extreme, students who failed to list either teacher under either question scored negatively on both sets of targets. This leaves for consideration the largest group of subjects, those who listed either or both teachers under Question 2 as "also preferred." In this division of the data where X was agent, the positive scoring was maintained when this teacher was listed and negative scoring when she was not. With agent Y, however, the relationship broke down. The general impression given by the results is that the relation between attitude and scoring were more stable for teacher X than for teacher Y. This is brought out in the two fourfold tables showing the ESP-attitude relationship for each agent (Table 2) as well as by a general inspection of the results for each agent as shown in Table 1.

This observation from the present experiment, as well as similar findings from the other A-W work in the classroom, suggests that a special study of the role of the teacher in the ESP situation might be worth while. Although almost all teachers taking part in the school tests contribute to some extent to the over-all total correlation between ESP score and attitudes, some teachers have obtained a much more pronounced separation than others.

Another dimension of ESP performance introduced by the role of the teacher has been noticed not only in the A-W work but in

that of Van Busschbach as well. Some teachers seemingly obtain significant positive (or negative) deviations for their class as a whole. For example, in the present experiment only the ESP results on the targets of teacher Y considered as a whole deviated from mean chance expectation to any appreciable extent (the deviation was −47 for 255 runs). But this is a study which should be carried out systematically through all the available data.

The same far-reaching implications for educational psychology as were presented by the earlier work are clearly suggested by the present exploratory tests.

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# TESTS OF CLAIRVOYANCE IN A MAN-DOG RELATIONSHIP

By GEORGE H. WOOD AND REMI J. CADORET

**ABSTRACT:** The clairvoyance tests described here were carried out by a man and his nine-year-old dog working together. The dog had previously been trained to answer questions by "pawing" a certain number of times on the arm of the person who asked him the question. In the experiments, ESP cards were used, and a code of numbers was adopted to correspond to the five ESP symbols. In the first group of tests, the man and the dog worked alone. The conditions varied from informal tests in which both participants could see the backs of the cards to more carefully guarded tests in which the cards were enclosed in black envelopes to prevent the possibility of sensory cues. Significant scoring was obtained under both conditions, with no falling-off of results as the possibility of sensory cues was eliminated. In another group of 20 runs with enclosed cards and a second person recording, the results were highly significant by themselves. A third group of tests with the cards sometimes enclosed and sometimes not were carried out in the presence of a member of the Parapsychology Laboratory. The results were generally negative for both conditions, those with concealed targets approaching statistical significance.

Even though the work gave evidence of ESP under good experimental conditions, it is not possible to determine from the data presented what part the dog had in producing the results and what part the man had. The authors make some suggestions for separating the two roles in further tests.

Mr. Wood is a chemical engineer working in the textile manufacturing field. Dr. Cadoret, who has published articles previously in the Journal, is a member of the research staff of the Parapsychology Laboratory.—Ed.

OVER THE YEARS, many puzzling anecdotes and reports have circulated concerning the ability of certain trained animals—usually dogs but occasionally horses—to show intelligent response to a variety of questions. The pattern of this behavior has been so complex as to raise the question in many people's minds whether these unusual animals can "think," in the sense that this vague word is used to describe certain types of human activity such as making mathematical calculations, reading, or even carrying on conversations (2).

The average animal case, as reported, usually concerns a dog or horse that has learned to signal by "pawing" or barking. For example, some simple code may be worked out and taught to the animal, such as one bark to signify "Yes," and two to signify "No."

Or the animal may point to one letter or number out of an alphabetical or numerical array. Reports of such animal performance include many instances when the animal was given a complex arithmetic problem or was asked to spell a word, and indicated the correct answer.

Suggested explanations of such apparently complex behavior have ranged widely, and a few interpretations are indicated here to demonstrate this range:

1. The animals can actually perform the mental operations required in reading, calculation, etc., and signal the appropriate answer (2).

2. Sensory cues denoting the correct response are given, either consciously or unconsciously, by the person working with the animal. This is the hypothesis usually advanced by students of animal behavior (2).

3. Some form of ESP is used by the animal to arrive at the correct answer to a problem. This last hypothesis has been suggested by those cases in which the animal seemed to answer correctly even though the experimental conditions eliminated the possibility of sensory cues. For example, the exploratory studies of Bechterev (1), who worked with highly trained performing dogs, and the experiments of the Rhines (6), who investigated the horse, Lady, give some support for an extrasensory element in the performance of these animals. In addition, the more recent study by Osis (3) suggests a psi relationship between experimenter and the cats used in the tests, and that of Osis and Foster (4) attempts to isolate an ESP factor in the cat.

It is extremely doubtful if any one of these hypotheses by itself is a sufficient explanation of all of the complex behavior exhibited by these animals. It is possible, though, to define experimental conditions sufficiently to determine the extent to which ESP can be demonstrated when the animal is allowed to work in much its natural way. By this we mean that when an animal is allowed to indicate answers under its usual working conditions, it is possible to set up a test so that ESP ability is required either on the part of the animal or the person working with the animal to achieve extrachance scoring.

It will be the purpose of this paper to describe experiments with a dog that exhibits the type of behavior previously described in reports on *apparent* arithmetic calculations, spelling, etc. We will present quantitative evidence bearing on the possible role of ESP in explaining certain aspects of this animal's behavior. It is not our intention to investigate at present the question of whether the animal can cipher, spell, read, etc. independently of human agency, nor will this paper attempt to elucidate the nature of possible sensory cues which may be involved in the animal's response. This is not to underestimate the importance of these questions, nor their relevance even to the ESP studies to be presented here; these are questions we leave for later work.

The past history of this dog as supplied by G.H.W. is as follows: Chris is a nine-year-old male mongrel belonging to G.H.W., one of the authors, who obtained the dog in 1949 when it was one year old. Chris's parentage is uncertain, but it is thought that his mother was a beagle.

The dog exhibited no unusual or outstanding behavior until August, 1953. At that time visitors brought to G.H.W.'s home a dog trained to indicate the numbers 1 to 14 by pawing an appropriate number of times. After the dog had performed, its owner turned to Chris, who was present, and asked "How much is 2 + 2?" Chris spontaneously pawed four times. That evening when Chris's master, G.H.W., returned from work, he repeated the identical question and received the same answer. Within a few days the dog had learned to indicate numbers from 1 to 10 to G.H.W. and seemed to be able to demonstrate the correct answer to simple problems in arithmetic. During the following few months Chris learned also to indicate any number by pawing each digit and to indicate letters of the alphabet by a number code. By this means he would give answers to complex arithmetic problems and to questions requiring the spelling of words.

In general, Chris's method of response (then as now) was to paw the answer on the arm of the person who was working with him. After each answer he was usually rewarded by some food: a bit of meat or a small piece of dog biscuit. He has worked successfully in this way over a number of years, not only with G.H.W. but also

with persons other than his master. There are at least eight people who have achieved similar results in having the dog spell, cipher, etc. while working with him when G.H.W. was not present.

Over a four-year period Chris has had approximately 60 public appearances, including several TV shows. His usual performance lasts about one hour and includes a demonstration of unrehearsed answers to mathematical problems and to questions put to him by his master and by people from the audience. All performances have been on an amateur basis (without fees) for various churches, charity groups, and service organizations. In general, the answer to any question or problem has usually been known by someone present at the time the dog gave the answer, but an occasional exceptional performance has raised the question of the dog's ESP ability. For example, sometimes the information demonstrated in a performance has presumably not been available normally to any person present. On one occasion several years ago, Chris indicated the correct score and some other correct details about a baseball game that had just been played; but no one present, presumably, knew the outcome at the time the dog gave the answers. In other cases, only one person present in a very large number of people may have known the correct answer to a question. For example, at a recent sales conference in Rhode Island at which men from all parts of the U.S.A. were present, a person from a Western state asked Chris the number of children he had and what the sum of their ages was. Chris indicated the correct answer to both parts of the question to his master, G.H.W. The questioner was about 40 feet away from Chris and G.H.W. in a large, dimly-lighted room containing 49 people.

## Preliminary ESP Tests

As long ago as 1954, instances of this general type raised the question of the possible role of ESP in Chris's performance. Accordingly, G.H.W. was approached by the Parapsychology Laboratory of Duke University, and as a result he began conducting ESP tests with Chris with both the ESP cards and ordinary playing cards. G.H.W. conducted these early exploratory tests along simple lines, generally "asking" the dog to identify the top card of a face-down, shuffled pack of ESP cards. As soon as the animal indicated his choice by a number code (for example, 1 "paw" = circle; 2 "paws"

= cross, etc.) the card was turned over and recorded as a hit or miss. Mr. Wood reports that the dog would achieve as many as 15 hits out of 20 trials on certain occasions with ESP cards. This is an excellent score despite the changing p value of the target (due to the use of the closed pack and the immediate trial-by-trial check of the results). However, because the tests were exploratory and the conditions were not strictly safeguarded, no conclusions can be drawn concerning the occurrence of ESP.

These tests were soon discontinued because G.H.W.'s time was limited and external demands channelled the dog's performance into more dramatic types of demonstrations. The Parapsychology Laboratory authorized a student from a neighboring university to investigate Chris, but unfortunately no ESP testing was done at that time, and only a few attempts were made to determine the nature of possible sensory cues in the dog's performance. At this stage the investigation was interrupted because of the difficulties imposed by distance and other circumstances.

### Results of Card-Calling Experiments, 1957-1958

With a changing outlook on dealing with the problem of psi in animals, the Laboratory re-established contact with G.H.W., and in September of 1957, R.J.C., a member of the Laboratory, visited the Woods. He observed the way the dog responded to questions, and found out that, in tests done three years previously, very high positive scoring had occurred in clairvoyance-type tests with standard ESP cards. It seemed wise, therefore, to start again with the clairvoyance conditions, which would possess the advantage of quick and easy control over the ESP conditions that GESP tests could not permit. At the time of this initial visit, several exploratory runs through an ESP deck were carried out. They involved modifications of DT and BT procedure. In general, the dog would be asked by his master what a particular card was, and the dog would respond by pawing from 1 to 5 times to correspond to a particular ESP symbol. After most calls the dog received a food reward.

In this first visit it was apparent that the dog could cooperate with the master in tests of this type. Accordingly, R.J.C. worked out a series of ESP tests which G.H.W. agreed to undertake after R.J.C. had left. At first the dog and master were to work under

informal conditions allowing both to see the backs of the cards. For example, one method consisted of spreading 10 cards from a shuffled ESP deck face-down on the floor and having the dog identify them one by one. Gradually it was planned to introduce more and more precautions against sensory cues from the cards (such as using the DT procedure or having the cards enclosed in individual opaque envelopes). The recording was done as follows: the responses made by the dog were recorded by G.H.W. as soon as they were made. All records were kept in a bound notebook in chronological order. G.H.W. generally checked up the card order at the conclusion of each run or part thereof (if less than the standard 25 calls had been

Table 1

RESULTS OF TESTS CARRIED OUT WHEN R.J.C. WAS NOT PRESENT

| Description of Conditions | Calls | Hits | Dev. | CR | P |
|---|---|---|---|---|---|
| **Results When the Backs of the Cards Were Exposed** | | | | | |
| 1. BT calls of top 10 cards of shuffled ESP deck laid on floor face down. Checked after all 10 calls made. | 550 | 173 | +63 | 6.7 | <.000001 |
| 2. BT with exposed backs of cards. Checked after 25 calls made. | 425 | 128 | +43 | 5.2 | <.000001 |
| 3. BT calls of varying numbers of cards (5 to 10) from the top of the deck. Then checked and procedure continued until 25 called. P≠1/5* | 225 | 74 | +29 | | |
| 4. BT single trials through the deck. Checked after each call. P≠1/5.* | 25 | 9 | + 4 | | |
| **Results When the Backs of the Cards Were Not Exposed** | | | | | |
| 1. BT. Cards in opaque envelopes. Checked after 25 calls were made. | 100 | 31 | +11 | 2.75 | .006 |
| 2. BT. Two decks shuffled together, each card in opaque envelope; set aside as called. Checked after 50 calls made. | 200 | 55 | +15 | 2.65 | .008 |
| 3. DT. One deck. Each card in opaque envelope. Checked after 25 calls. | 1125 | 324 | +99 | 7.38 | <.000001 |
| 4. DT. Two decks shuffled together, each card in opaque envelope. Checked after 25 calls. | 150 | 45 | +15 | 3.1 | .002 |
| 5. DT. Exposed deck on floor, face down. Checked after 25 calls. | 250 | 56 | + 6 | 0.95 | .34 |

*For trials done in this manner, the probability of a hit is not equal to 1/5. Hence, mean chance expectation and the variance are not calculable and the deviation is of suggestive value only.

made), and the complete pack was reshuffled before the next series of trials.

Table 1 shows the results of the clairvoyance tests conducted by G.H.W., during this period and later, when R.J.C. was not present. (The only data left out are a special series of 20 runs which are given in Table 2.) This first table is separated into two parts: the calls made when the backs of the individual cards were exposed to the experimental participants, and those made under conditions precluding sensory cues from the cards. Significant above-chance results were obtained by G.H.W. with each condition. There appears to be no falling-off of scores as the sensory cues from the cards were more and more effectually eliminated according to the changes in procedure described in the first column of the table.

When it became apparent, after several months of testing, that high scoring could be maintained despite the elimination of sensory cues from the target cards, R.J.C. proposed a special series to be carried out by G.H.W. This series, which took place between December 4 and December 16, 1957, consisted of 20 runs collected consecutively by one standard method with special precautions in the preparation of the target order and recording. The method was the DT technique with the cards sealed in individual opaque envelopes. About two runs were done per evening. Target decks were prepared as follows:

Cards and envelopes were shuffled separately, usually by a third person, out of sight of the experimenter (G.H.W.) and Chris. The cards were placed in the individual envelopes without being looked at and were then reshuffled. These packs were prepared the night before the tests and were concealed in boxes until just before the test was made, when the pack was removed from the box, cut by G.H.W., secured by an elastic, and placed on the floor before the dog. G.H.W. recorded each guess in his bound notebook as it was made. The third person present during all the series (either G.H.W.'s wife or a friend, Miss Rosemary Goulding) would remove the cards from their envelopes and call out the symbols to G.H.W., who recorded them in the notebook, keeping the call order covered at the time. In this way, one to two runs (usually two) per evening were collected until the predetermined number of runs was finished. The results of

Table 2

RESULTS OF SPECIAL SERIES OF TWENTY DT RUNS

| Description of Conditions | Calls | Hits | Dev. | CR | P |
|---|---|---|---|---|---|
| Witnessed Runs. Series of predetermined length. Sensory cues from cards precluded... | 500 | 152 | +52 | 5.8 | < .000001 |

this series, which are shown in Table 2, are highly significant by themselves and are of the same order of magnitude as the results shown in the earlier work given in Table 1.

In October, 1957, and again in January, 1958, R.J.C. made visits totaling 10 days for the purpose of testing Chris. During these visits it was a general rule to test the dog once a day at the time G.H.W. usually conducted his tests (in the evening) although more runs were

Table 3

RESULTS OF TESTS DURING TWO VISITS WHEN G.H.W. WAS EXPERIMENTER AND R.J.C. WAS OBSERVER

| Description of Conditions | Calls | Hits | Dev. | CR | P |
|---|---|---|---|---|---|
| **Results of Visit of October, 1957** | | | | | |
| 1. DT. Some decks sealed in envelopes. Backs of cards not visible to dog or master.* | 525 | 85 | −20 | 2.18 | .03 |
| 2. BT. Cards handled one by one. Dog and master able to see backs of cards. | 225 | 41 | − 4 | | |
| 3. BT. Cards concealed in individual sealed black envelopes.* | 50 | 13 | + 3 | | |
| **Results of Visit of January, 1958** | | | | | |
| 1. DT. Cards concealed in individual black envelopes.* | 250 | 49 | − 1 | | |
| 2. BT. Cards concealed in individual black envelopes* | 254 | 36 | −14.8 | 2.32 | .02 |
| Total, both visits | 1304 | 224 | −36.8 | 2.55 | .011 |
| Total for runs collected under conditions when sensory cues from cards were prevented | 1079 | 183 | −32.8 | 2.50 | .012 |

*Conditions controlling sensory cues from backs of cards.

collected per session than was the custom. Otherwise, R.J.C. tried to keep the conditions as close as possible to those ordinarily followed by G.H.W. and to act merely as an observer, occasionally handling the cards but never working directly with the animal. The data collected during both these visits are shown in Table 3.

During the October visit, several conditions of calling were used in an attempt to find out what method seemed most successful. As can be seen from the table, the results in general are below the chance level, with a marked negative deviation for the BT condition in which there were controls against sensory cues. During the second visit, in January, 1958, sensory cues from the cards were rigidly excluded, and this negative tendency is again evident, the total deviation for both visits being significant at the two percent level. No one method of calling seems to be implicated in this negative deviation. An interpretation of this below-chance scoring will be offered in the discussion section.

## Discussion

The results show evidence of ESP occurring under good clairvoyance experimental conditions involving some type of man-dog-target interaction.

These preliminary experiments serve to re-emphasize the importance of a question which has faced parapsychology since its inception as a scientific study; namely, What are the roles of the subject and experimenter (or "percipient" and "agent") in the ESP process? Applying this general question to the present situation, just what role in producing results does the man play and what exactly does the dog accomplish? In an ordinary ESP test involving a human subject and experimenter, the question of who "has" or who "produces" the ESP has never been clearly defined, at least not under telepathy conditions. When only clairvoyance can operate, the role of the subject seems to be the determinative one, although even then the experimenter can have at least an indirect effect on the results.

To sharpen our focus on this important question we shall briefly discuss the main interpretations of the present experiment that occur to us and suggest possible ways to differentiate them in future experiments on the basis of available evidence.

1. The person working with the dog is the actual ESP subject and cues the dog sensorially (but unconsciously).

2. The dog himself initiates the answers in a way similar to that we assume gifted human ESP subjects do.

The first hypothesis might seem to be favored by the marked difference in the sign of the deviation between the results obtained by G.H.W. working alone or with observers other than R.J.C., and those obtained with R.J.C. as observer. Though the cause of such contrasting results cannot, of course, be stated with certainty, it is interesting to observe that psi-missing has occurred before in situations in which the subject might possibly be under a mental strain because of the "official" nature of the testing (5).

Evidence which might favor the second hypothesis comes from the work of three other persons who carried out clairvoyance tests with the dog (although the master was in the same room). There are eight of these runs and the total deviation is +8. From this we can draw no conclusion; but if the dog were to be significantly successful in ESP with a number of different people, it would tend to support the second interpretation, especially if in contemporaneous tests these experimenters could demonstrate no ESP themselves. But before any conclusion could be reached on the strength of such evidence, it would be necessary to show that the human subjects were as favorably motivated for ESP performance when working alone as they were with the dog. This might be very difficult to do.

Another approach which should be explored is to try to train the dog to indicate his answer to a machine of some type (for example, a wooden "arm" which he could scratch with no one present to give any cues). If the dog could be trained to work under such conditions, this should greatly facilitate tests that would help to reach a more definite interpretation of the results.

We hope that further work along these lines will throw some light on the functioning of ESP in animal cases as well as help us understand better the psi process in human beings.

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# COMMENTS AND LETTERS

## Parapsychology and Time: Comments on Dr. Kooy's Paper "Space, Time, and Consciousness"[1]

I very much hope that Dr. Kooy's paper, which appeared in the December number of the *Journal,* will receive the serious attention of parapsychologists and philosophers interested in the wider issues of science. If, as he has argued, precognition and retrocognition follow as natural corollaries from a geometry of space-time, parapsychologists must, in all good faith, come to terms with mathematical physics. But if, as I have argued in various papers and articles (**1-17**), the issues are those of an extremely complex and highly debatable *philosophy of science* going beyond the now fashionable syntactics, semantics and pragmatics, the parapsychologist cannot and must not capitulate to the mathematical physicist, at least in the present state of human knowledge.

The geometry of space-time, like any geometry, can be axiomatized in several different ways by the use of different "indefinables" or primitives. It may seem that altering a single term in the geometrical equations sometimes leads to revolutionary physical and philosophical consequences. We should, nevertheless, guard against confusing mathematical curiosities with reasonable and acceptable axiomatizations of the topology of time. I contend that precognition and retrocognition, assuming that they occur at all, render highly problematical the status of "past-present-future" viewed against the background of much current relativistic and quantum physics, statistical thermodynamics and "information theory" (see my summing-up in **7**). I have discussed elsewhere the physico-mathematical issues, especially those which Dr. Kooy raises. It will be sufficient if I single out here two highly controversial presuppositions of his

[1] I am grateful to Mrs. Eileen J. Garrett and Dr. J. B. Rhine for the encouragement I received from them in my studies. I thank Mr. G. Zorab for forwarding to me a mathematical appendix to Dr. Kooy's paper and Dr. Adolf Grünbaum of Lehigh University, Bethlehem, Pa., U.S.A., for sending me a revised draft of his paper appearing in *The Philosophy of Rudolf Carnap,* edited by P. A. Schilpp in the *Library of Living Philosophers,* Vol. 10.

hypothesis about precognition and retrocognition. First, does contemporary physics rule out as inadmissible the notion of a "real becoming" in the universe? Second, is Dr. Kooy's suggested explanation of our awareness of "past-present-future," in the language of statistical thermodynamics (and possibly "information theory"), entirely consistent with his hypothesis of precognition?

I

In a previous article (**18**) as well as in his present paper, Dr. Kooy almost takes it for granted that the division of the world into a "past" and a "future" by a "present" that is "advancing" into the "future" has no valid application outside our ordinary consciousness; he suggests that this consciousness misreports reality in a radical fashion. The argument is not exactly new. It was sketched by G. T. Fechner in his ingenious "Paradoxes of Dr. Mises" and revived by C. H. Hinton and P. D. Ouspensky. Since then, a number of writers interested in parapsychology have toyed with it, notably Maeterlinck, Whately Smith (in his later life, Whately Carington), and Warner Allen (in his well-known trilogy). J. R. Smythies and Hornell Hart have proposed special adaptations of multidimensional spaces in which consciousness can "travel." I have discussed their hypotheses about "psychic spaces" in three recent articles (**11**, **12**, **17**). What is new about Dr. Kooy's version of the theory is his strenuous attempt to relate it to relativistic geometry and cosmology. He maintains that, in a world which welds together space and time, concepts like "motion" and "rest," "dynamic" and "static" are equally inapplicable and puerile. We remember that G. C. Barnard, in his book, *The Supernormal,* published nearly twenty-five years ago, suggested that the fallacies about "time travelling," in the theories of Hinton and Dunne and in the fantasy of H. G. Wells, stemmed from a colloquial use of the word "motion." To do justice to the relativistic fusion of space and time, Barnard said, we have to attach a new meaning to the expression "movement in time" instead of letting it connote "spatial displacement" in the ordinary sense (see the citations from Barnard in my article, **8**).

To come to Dr. Kooy's basic assumption: It may be said that the causal processes of classical mechanics are essentially reversible

and that the temporal order described by them is serial only in an *extrinsic* sense. A symmetrical causal relation holding between reversible processes may yield "temporal betweenness," but not a serial order which *intrinsically* distinguishes the direction "after" from the direction "before" (see my discussion **3** and **13**). Dr. Kooy is presumably of the opinion that the asymmetry of temporal "before-after" is sufficiently characterized by statistical thermodynamics and does not imply a "past-present-future" series. Disputes about the "reversibility of motions," even in classical physics, are by no means over. So distinguished a philosopher of science as K. R. Popper has recently, in *Nature,* questioned the widespread belief that classical mechanics is competent to describe only "reversible" phenomena; and R. Schlegel, in the same journal, has submitted that certain solutions of the equations of Newton's second law signify a lack of reversibility (see the citations in **13**).

Dr. Kooy seems to imply that the unification of space and time, in the geometry of special relativity, involves the dropping of a unique time-dimension. There are two separate questions to be asked here. Does the geometry of special relativity recognize a causal order of events possessing an *ordinal* structure determined by *temporal* "before-after"? Does it admit of a unique division into "past" and "future," a feature of our ordinary time which finds no analogy in space? The answer to the first question must, I think (*pace* Dr. Kooy), be in the affirmative. Einstein's distinction between the "before-cone" (*Nachkegel*) and the "after-cone" (*Vorkegel*) presupposes an *ordinal* structuring of events. The axiomatization of the topology of time achieved with the aid of "light-signals," which can be "flashed" into the "after" but not into the "before," definitely implies this feature. To the analyst the distinction between Minkowski's space-time and a Euclidean four-space may seem trivial, consisting merely in the substitution of an imaginary for a real variable in one of the co-ordinates; the device turns the co-ordinates into a homogeneous, orthogonal set and allows the four dimensions to be treated as the components of a vector and the laws of relativity to be handled by tensor analysis. But, as I have urged in several articles (**1**, **2**, **4**, **6**, **8**), the analytic simplification masks the epistemological issues and invites serious misconceptions. I have shown (**8**) how the statements of earlier writers on relativity like Langevin,

Carmichael, Tolman, Robb and Meyerson in effect conceded the uniqueness of the *ordering* of the time-dimension. G. Y. Rainich, in his *Mathematics of Relativity,* has dwelt on the very serious "complications" resulting from the use of a mathematically "imaginary" time-co-ordinate. He has observed that the time-dimension, in special relativity, can always be distinguished from the space-dimensions even when it is inseparably connected with them. Co-ordinate transformations never carry a "time-like vector" into a "space-like vector" or *vice versa.* C. Møller, in *The Theory of Relativity,* has warned us that "on the whole, the epistemological significance of the four-dimensional representation should not be exaggerated" and that, notwithstanding the "formal symmetry" between space and time in the geometry, there is "a fundamental physical difference between them" (see the citations in the postscript to my paper, **16**). However much we may dissent from the cosmological theories of E. A. Milne and his co-workers, we cannot be too grateful to them for their protest against the emotional exaggeration of Minkowski's celebrated statement: "Henceforth, space and time considered separately have vanished into the shadows and only a sort of blend of the two can be regarded as the sole reality" (see the appreciative estimates of Milne in **8**). Milne has told us that "to make an artificial construct, 'space-time,' out of our experiences and then say that we 'separate the continuum into two ingredients' is to move in a circle." Much popular talk about our "human spectacles" dividing "reality" into "space" and "time" has confused the philosophical issues. The particular construction which Dr. Kooy chooses to put on the geometry of special relativity is at least debatable.

Can it be said that general relativity and relativistic cosmology furnish unambiguous support to Dr. Kooy's speculation? His whole treatment in his two papers, of course, suggests the language of general relativity. Riemannian geometry enables us to express the "curvature" of a four-dimensional space-time by its array of $g$ coefficients. The "curvature" is a property intrinsically bound up with the algebraic form under which the $g$ coefficients group themselves. Dr. Kooy has rightly admonished us that we do not have to do here with any simple analogue of the "curvature" of a line in

a plane or in the space of perceptual experience. Tensor analysis is appropriate to the grouping of the $g$ coefficients in relativity. Dr. Kooy's factor $\sqrt{g'}$ depends on $x'$, $y'$, $z'$, $t'$ defining the space-time metric which, in turn, depends on the general function, say $F(x, y, z, t)$, taken as an integral extending throughout a "curved" and "closed" space-time.

I must content myself with a few observations here. A wide variety of "world-models" have been explored by Friedmann, Heckmann, Robertson, Milne, Whitrow and others (see **13**). Most cosmological solutions of Einstein's field equations, with non-vanishing density of matter, have the peculiarity that, in a certain sense, they posit an "absolute" time-co-ordinate. There exists in these "models" a system of three-spaces everywhere orthogonal to the "world-lines" of matter. Einstein's own early "cylindrical model" contained a fourth co-ordinate, $t$, explicit, isolated, free from all disturbances. De Sitter's early "model," by contrast, offered a curved "hyberbolic time" which, for any observer, would appear to "stand still" at an elusive "horizon"; but it ceased to be exciting when it turned out to be a *static* solution of the field equations which could not conceivably describe the real universe. In the newer models in which the metric is a function of *time*—Dr. Kooy has referred (**18**) to the "expanding models" of Eddington and Lemaître —the universe is obviously a *process*. In Milne's Kinematic Relativity, motions of particles, on one of two time-scales, are *essentially* irreversible; time has an "arrow" right from the start, without our juggling with thermodynamics. In the "new cosmologies" of Bondi, Gold and Hoyle, matter is supposed to be continuously created to compensate for the "thinning out" produced by "expansion." Although we cannot hope, at the present moment, that any triumphant conclusion about the "ultimate nature of time" will emerge from the welter of cosmologies, the over-all impression surely is that of a *temporal* universe (see my discussion in **13**).

There remain a few unorthodox "models." E. J. Öpik, in the *British Journal for the Philosophy of Science* and the *Irish Astronomical Journal* (see **13**), preferred a "closed, oscillating" universe to an irreversible process of temporal origin. He cautioned readers, however, that the "repetition" in his "closed" universe would not be

an "eternal recurrence of things" in Nietzsche's sense. The individual celestial bodies in successive phases of the universe would not be identical; nor their individual inhabitants. Kurt Gödel showed, in the *Reviews of Modern Physics* (see **13**), that Einstein's field equations of gravity can be made to yield solutions with *closed,* "time-like" lines. The *closed* lines make a difference to the *open* lines, too, so that a *unique* temporal ordering of *all* points is not possible in this "model." We cannot assume in it that $t$ necessarily *increases,* as we move in a "time-like direction." If $P$, $Q$ are any two points on a "world-line" of matter, and if $P$ precedes $Q$ on it, there exist "time-like" lines connecting $P$ and $Q$ on which $Q$ precedes $P$; i.e., it is *possible* in this weird "model" to "travel" into the "past" and "influence" it. Gödel has pointed out that the physical relevance of his "model" is far from demonstrated. Whether it will yield a "red shift" for distant objects and the observed irregularities in the distribution of the axes of the rotating galaxies remains a tantalizing question. Since the radius of the smallest "time-like" lines, in Gödel's model, is of the same order of magnitude as the world-radius in Einstein's early *static* model, the implications for *precognition* occurring in a *terrestrial* setting are not clear at all. Einstein declared that "it is interesting to weigh whether these [solutions proposed by Gödel] are not to be excluded on physical grounds." The animadversion would seem to apply to the "elliptic" time proposed by B. Abramenko in the *British Journal for the Philosophy of Science.* J. T. Davies, in the same journal, rejected, not without justification it seems to me, all hypotheses about "time-curvature" as "being a complication at present scientifically unwarranted." Since, in Abramenko's "model," the effects of "time curvature" are "usually insignificant" and become noticeable "only at large distances and long intervals," it is clear that the problem of precognition has not even been scratched. In the "world-model" which Dr. Kooy himself contemplates (**18**), the universe is a hypersphere; space and time are both "curved" on it; "expansion" corresponds to one "oscillation" of the "time" (see the technical details of the theory given in **13**). He deserves to be congratulated on exploring these logical and mathematical possibilities. But he will be the first to grant that the relevance of his "model" to the observational data of

cosmology and physics is not established; he has to contend with the rival tabernacles of other cosmologists, far too many of them. Michael Scriven has expressed grave doubts about the feasibility of drawing sweeping conclusions about the origin and destiny of the physical universe as a whole. When the *locus standi* of Dr. Kooy's hypothesis may be doubted in cosmology, can we be sure that it will secure a foothold in parapsychology where even the "facts" are sometimes disputed?

Does relativity exclude a "real becoming"? In an earlier article (**16**), I have quoted authorities who are convinced that relativistic physics and cosmology are *not incompatible* with the "past-present-future" series even if they do not seem to imply it *directly*. Eddington was one of those who subscribed to the view (see the citations from Eddington in **8**). Hans Reichenbach characterized the "present" as the "threshold moment" dividing an essentially *determinate* past from a basically *indeterminate* future (**16**). Bondi has said that, in a universe with a basic "indeterminacy," the "future" which is merely "statistical expectation" is transformed into real "occurrences." Norbert Wiener, in his book *I Am a Mathematician,* holds, on cybernetic grounds, that "becoming" is very likely a feature of the real universe. Even Hermann Weyl, who pronounced that "the objective world *is,*" granted, in his *Philosophy of Mathematics and Natural Science,* that "right" and "left" do not pose as basic a problem for science as "past" and "future" (see the citation from Weyl in **13**).

It is of interest in this connection to notice that G. D. Wassermann has sketched a hypothesis about precognition on the lines suggested by quantum field theory. The hypothesis does not commit us to a "pre-existing future." I have commented elsewhere (**15, 17**) on Wassermann's provisional sketch of his hypothesis. I agree with Michael Scriven that, in the present state of quantum field theory, biology and psychology, Wassermann's attempts to extend the use of familiar mathematical constructs (like the Lagrangian and the Hamiltonian) are far too ambitious, time-consuming, and unconvincing. Yet, as an alternative to Dr. Kooy's cosmology, Wassermann's hypothesis merits careful study; one must admire the immense knowledge of physics, biology and psychology deployed by

Wassermann even if one cannot agree with him on parapsychological questions.

If "becoming" is fundamentally a *topological* notion depending, for instance, on the increasing number of interacting sets and subsets of events in the universe—our "memories" are but conspicuous examples of such sets—I do not see that a *metric* geometry of the kind to which relativity commits us can easily accommodate the idea. It may be said that all attempts to *define* "presentness" (even Reichenbach's characterization of it) fail to yield a unique "now" distinguishable from other "nows." But then, as C. D. Broad has shown illuminatingly, all attempts to define the *tenses* in a language are necessarily circular. The temporal copulas behave almost like "weak tautologies" which cannot be defined or got rid of. Milne and Bridgman have maintained that our awareness of "passage" must be regarded as operationally primitive in physical theory (see my postulational approach in **3**). Whether or not we agree with them, we have to distinguish technical constructs of different orders. There are no good reasons for supposing that the symbol *t*, which occurs in relativistic cosmology or quantum field theory, will necessarily have all the characteristic features of the time of awareness with which psychology and parapsychology are intimately concerned. However much Bergson's *la durée réelle* may savor of a romantic exaggeration, not all can acquiesce in Dr. Kooy's peremptory dismissal of "becoming" from "reality."

II

I turn to Dr. Kooy's proposals for explaining our ordinary awareness of "becoming" on statistical and thermodynamic grounds. He argues (**18**, especially pp. 142-44) that the privileged "direction of time" is only that of "entropy increase." I confess that I find his treatment of the problem insufficient; perhaps he has been hampered by considerations of space. He does not mention the technical difficulties cropping up from the classical ergodic hypothesis, Von Neumann's quasi-ergodic hypothesis and Von Mises' pseudo-ergodic hypothesis (see my discussion in **13**). The researches of Reichenbach and Grünbaum have shown that the entropic behavior of a single, permanently closed system does not suffice to define a time-direction; we have to rely rather on the entropic behavior of

a majority of quasi-closed systems. Attempts—such as those of Max Born and Landé (see **16**)—to *deduce* thermodynamics from the *fundamental* equations of quantum mechanics are still beset with difficulties. We may, if we choose, replace quantum states by projective operators in a Hilbert space. The difficulty, then, is to show that the "transition probabilities" are related to the scalar product of the unit vectors representing different quantum states. Dr. Kooy's discussion of "transition probabilities" (**18**, p. 147) is perfunctory. Does the law of entropy afford a clue to the problems of biological organization? Martin J. Klein has remarked, in the *British Journal for the Philosophy of Science,* that "degree of order" measured by entropy does not necessarily correspond to "organization" in the biologist's sense. Discussing the relation of thermodynamics to the subjective "sense of time" in the same journal, K. G. Denbigh of Cambridge University remarked that the "emergence of spirit" in living creatures suggests "tendencies" which are "outside the scope" of the second law of thermodynamics though not perhaps "contrary" to it. In the "parallel" but "more hopeful" processes of psychology, "the significance of time is not the same as it is in science but is related to volition and purpose."

The researches of Wiener, Shannon and Brillouin suggest the equivalence of entropy and "amount of information" in a wide variety of situations including the mythical case of Maxwell's "demon" which can sort out molecules. None the less, the "average unexpectedness" (or "improbability") of a signal defined by Wiener and Shannon—as, say, $\Sigma\ p_i \log \left(\frac{1}{p_i}\right)$—is not an absolute quantity but a function characterized in relation to a chosen distribution of prior probabilities or "ensemble" of events. In physics the distribution may be fixed by entropy; but it may be determined far otherwise in other contexts. Scruples may be raised about the *direct* relation of Shannon's measure not only to physical entropy, but to the privileged "direction" of the time of awareness. Brillouin sometimes tends to equate the mathematical "unexpectedness" of "information" with the "amount of information." Shannon has warned us that the "improbability" of a "signal" need have no direct connection with its logical content. I do not think that the questions about time

which arise in parapsychology can be settled on the grounds furnished by "information theory" alone.

Let us grant, however, for the sake of argument, that statistical thermodynamics and modern "information theory" can elucidate the "past-present-future" series as a *macroscopic* or large-scale phenomenon. Is not Dr. Kooy asking us to pledge ourselves to a philosophic naturalism which regards ordinary human consciousness as strictly isomorphic with physical processes and brain states characterized by statistical thermodynamics? Is the assumption altogether compatible with Dr. Kooy's hypothesis of precognition? In a previous article (**16**, especially pp. 175-76), I have stressed a difficulty which Grünbaum's brilliant researches have dragged to light. Grünbaum has explained how a "record" and a "memory trace" can both be treated as negative entropy or "negentropy" acquired at the expense of the environment of which they are parts. The production of a state of locally greater order (lower entropy), like the imprint on the beach discussed by Schlick (**16**, p. 175), by interaction with the outside world, involves a more than compensatory entropy increase in the larger external system. Grünbaum has furnished reasons for supposing that, consistently with the postulates of thermodynamics and "information theory," we can "record" what is "past" in relation to a particular "here-now" and *not what is "future."* He has argued, cogently it appears to me, that the conclusion holds even on the deterministic assumption of "pre-existing world-lines" in the universe. The philosopher J. J. C. Smart, in recent papers, has drawn our attention to the curious situation that whereas, from a limited region of space, we can deduce a great deal of the *past* history, to *predict* similar facts about the *future,* even a Laplacean super-intelligence would have to consider initial conditions over a *very wide* region of space. In a recent address delivered on the occasion of the anniversary of the Indian Statistical Institute at Calcutta, the Soviet academician N. N. Bogolyubov showed that if we have to do with a particle or system having a small number of co-ordinates, the condition of which at some instant of time is fully known, but which is subject to interaction with a system having many co-ordinates, statements about which can be made only in the language of probability, predictions over comparatively longer

periods of time would have to reckon with the uncertainties of the larger system.

The fact that precognition can be *recorded* (as a "guess") by the subject in the laboratory and can sometimes be *acted upon* by the percipient in the spontaneous situation creates an impasse for physics. Some spontaneous precognitions seem to range over days and months, if not years, of human lives with their extreme complexities. How does Dr. Kooy account for the anomaly within the present framework of the entropic hypothesis and "information theory"? Unless I am mistaken, he introduces a hypothesis which is at variance with, and even imperils, the philosophic naturalism which he virtually espoused earlier. He postulates something like a "subconscious insight" (**18**, p. 137)—or is it a "collective consciousness"? (**18**, p. 133)—capable of ranging over the "world-lines" in his "closed" and "curved" space-time and possibly calculating the $\sqrt{g'}$ determined by the metric and by the function $F\ (x, y, z, t)$ taken as an integral extending throughout space-time. The assumption goes far beyond the wildest "depth psychology" of today; the modest theories of Servadio, Eisenbud, and Ehrenwald will not bear comparison with it. From all that Dr. Kooy says, the "subconscious insight" (or "collective consciousness") is not restricted by "past-present-future." How, then, can it *transmit* its knowledge (or its "awareness") to the ordinary consciousness and cause it to emerge as a "precognition" or "premonition"? Many cases of spontaneous precognition seem to argue overwhelmingly for a *selection* of what is *transmitted*. Ehrenwald has studied the problem from the psychoanalytic angle. How are we to conceive of the "selection" and "transmission," at the "deeper levels" of the "self" or "mind," if not in terms of *some* kind of time? Little is gained by brusquely condemning the language of tenses as inept. The White Queen's testimony, in *Through the Looking-Glass,* that there is great advantage in a memory which "works both ways," allowing the trial to commence *before* the crime and the finger to bleed *before* it is pricked by a needle, is comforting and edifying; but it will not do in a serious discussion of precognition. What is needed is an understanding of the *emergence* of precognitive insight *at a particular instant of "clock-time" in an empirical life-history.* The difficulty has compelled many exponents of the "fourth

dimension" to assume that the "deeper consciousness" can "travel to and fro" along "time-like" lines. I own that I do not understand how a "world-line" which *is* the entire history of a particle or observer and *does not become* can justify the bungling metaphor of a "sub-consciousness" which "crawls" or "creeps" along the "past" and the "future." G. C. Barnard, in facing the puzzle of how precognition *intervenes* in ordinary time, lapsed into the crude "spatialization" he had censured earlier as out of keeping with relativity (see my criticism of Barnard, **8**). Dr. Kooy himself has provided spatial diagrams to make precognition and retrocognition more plausible.

Let us consider the hypothetical instance in which "A's dream" is "projected" into "B's future." Dr. Kooy postulates a "midlevel" region (of mind?), AB, which is neither A nor B but of which A and B are indispensable and inseparable components. How does the "midlevel" region, which knows nothing of "past-present-future" (Dr. Kooy assures us that episodes like "birth" and "death" are irrelevant to it), facilitate the *emergence* of a "phantom" or a "precognition" in *B's life-history?* The problem of the *interaction* (or is it "introaction"?) of the "midlevel" region with A and B seems intractable. I doubt whether anything that Dr. Kooy may have to say will serve to make the scheme intelligible. He reminds us that G. N. M. Tyrrell used the term "midlevel" region. So far as I am aware, Tyrrell was never tired of insisting that precognition breaks all our categories. Dr. Kooy is obviously not prepared to go so far, notwithstanding his sympathy for mysticism and the Hindu *"Tat tvam asi"* ("That thou art"). He regards mathematical physics as sacrosanct. Ehrenwald's wise agnosticism about the "metalogical unconscious" seems salutary by contrast.

Dunne met the problem of "precognitive intervention" in human lives by boldly invoking a multidimensional time. Dr. Kooy denounces the hypothesis as arbitrary and unjustified. Perhaps not all can follow him. Pascual Jordan, in a paper he read at the Conference in St. Paul de Vence, referred appreciatively to the suggestion about a two-dimensional time thrown out by Wenzl and said that it deserved careful consideration. I gather that Martiny has proposed four of these temporal dimensions. In the *British Journal for the Philosophy of Science,* H. A. C. Dobbs, C. D. Broad, and

C. W. K. Mundle have discussed the implications of a two-dimensional time. In the same journal recently (**14**), I have sketched a topological generalization of Dunne's theory. I see no reason why we should not recast along these lines the hypothesis of an "extended present" proposed by H. F. Saltmarsh (see my discussion in **5**) and commended recently by K. G. Denbigh in the *Journal of the S.P.R.*

Dr. Kooy brushes aside "survival" as an illegitimate hypothesis. To me it is not at all self-evident that "precognition" and "survival" are *mutually exclusive* hypotheses. Many alleged cases of "communicators" mention the "precognitive insight" of the "discarnate" (see **5**). May not the "deeper self" or the "subconscious" inhabit an ampler environment containing "surviving egos"? Although I cannot hope to go into all the technicalities here, the topological generalization of a multidimensional time I have sketched (**14**) *could* accommodate the queer possibility. We need only postulate that the *necessary* (if not the sufficient) condition of the "telepathic interaction" of selves is the intersection of the "epochs" of their histories, not in the familiar time but in the *multidimensional extensions of it.* In principle at least, we can deal with cases of the type discussed by Dr. Kooy: the ordinary "telepathic situation" in which the empirical egos are contemporaneous in the familar time-dimension and the precognitive situation in which the events foretold occur *after* the death of the "seer" (as in the alleged case of Sonrel reported by Richet and Osty). We can also allow for something which Dr. Kooy disallows: the "telepathic interaction" between the "discarnate" and the "incarnate" as well as the "telepathic interaction" between the "discarnate." I realize that it is exceedingly dangerous for the parapsychologist to mistake a possibility for a probability. I maintain, however, that it is no less dangerous for him to dismiss a possibility on purely *a priori* grounds. The pioneering investigations of Dr. and Mrs. Rhine should teach us to weigh *all* possibilities without dismissing *any* of them hastily. Dr. Karlis Osis, the Director of Research of the Parapsychology Foundation, is launching a new inquiry into the "mediumistic phenomena." I agree with philosophers like C. D. Broad, H. H. Price, and C. J. Ducasse that the inquiry is worth while.

Dr. Kooy thinks that a "psychological time" is an unwarranted

hypostatization. He would rather suppose that our ordinary awareness of time is sufficiently explained by its cerebral correlates. There is no unanimity among neuro-physiologists about the supposed cerebral correlates of the "time sense" (see my discussion in **9**). Grey Walter and others have interpreted the "alpha rhythm" of the brain in terms of a "scanning mechanism," on the analogy provided by a television set. The interpretation raises some thorny questions which have not yet been answered (see my discussion in **17**). Lashley has remarked, in the Hixon Symposium on cerebral mechanisms, that the role of time in cerebral co-ordination is still very obscure and that hypotheses about time as the "fourth co-ordinate" do not help very much. I am afraid that Dr. Kooy's proposed research into "brain rhythms" will not provide an *experimentum crucis*.

Many features of our awareness of time await experimental probing. Nuances of interest, motive, emotional stress and volitional tension often seem to color our estimates of the passage of time (see **10**). Individually consistent over-estimations and under-estimations of time probably occur; Rorschach records hint at them. Different individuals, and possibly different social classes, may differ in their characteristic psychological attitudes to "past" and "future." And although the psychoanalytic account of the alleged "dilatations" and "contractions" in the field of temporal awareness must be treated with reserve, it is yet a forcible pointer to the presumably psychological determinants of temporal estimates. Our conscious appreciation of time partakes more of the character of a judgment than of a simple perception. One promising approach to the problems of precognition and retrocognition is afforded by the study of the *psychological* bases of temporal awareness. I do not doubt that many puzzles about our ordinary awareness of time have to be solved before we can vary purposefully the conditions in a precognitive experiment and learn more about this most enigmatic of all the manifestations of ESP. Meanwhile Dr. Kooy has done well to explore some logico-mathematical possibilities of relativity. If I have differed from him, sometimes sharply, it is in the hope that speculation in this obscure field will be advanced more by discussion and mutual criticism than by dogmatic assertion.

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## Reply to Dr. Chari

I am very pleased that Dr. Chari has paid such serious attention to my article, "Space, Time, and Consciousness" in the December 1957 issue of this *Journal.* After having given careful consideration to his comments, I have the impression that there is only a question of difference of background of thought. In view of the restricted space I have at my disposal, let us confine attention to some major points which may be of special parapsychological interest.

Obviously Dr. Chari has the misleading impression that I regard mathematical physics as sacrosanct, and am not prepared to accept anything beyond it. I especially agree with Dr. Chari that even the most advanced ideas about what we call "the physical world" are quite inadequate to *explain* precognition, any more than the general theory of relativity "explains" gravitation. We must begin by considering this very mysterious phenomenon with an open mind. Then the situation can be shortly described as follows: When we see—travelling through terra incognita—a mountain peak at a great distance, piercing through a cover of clouds, we conclude that there must be a large mountain of which only the summit is visible to us. Nobody will assume that there is *only* this mountain peak, floating in space—that the mountain base is still in the process of formation and comes to completion just at the moment we arrive there. In a similar way, if some person "sees" in a precognitive flash some future scene, containing noninferential details such as the falling of a meteor through the roof of a certain house, it is plausible to assume that this future scene—in its quality of shadow which *may* be cast before—does not "exist in itself," but is only a small part of the whole of events.

At least the future scenes which "are not seen" and which are inevitably connected with the small part emerging in the precognitive flash must have the same degree of reality. In opposition to such precognitive flashes which exclude any kind of human inference (even accepting telepathic information in the most extensive way) and which also exclude any kind of subconscious action to *realize* the precognitive flash, we can only repeat the words of Richet: "L'avenir existe, car le voyant l'a vu." Being aware of this bewildering situation and discarding all prejudices due to common sense and to

familiar, cherished convictions, only the most embracing concept can be an adequate, safe starting-point for constructive thinking. This most embracing concept is the *being* of events, in a nondynamical and also in a nonstatistical sense.

Even if we should accept all precognition as due to the inference of a Laplacean universal spirit, this special situation would be included by the most general concept of *being*. For the events are identical with the relations between the events. To derive events by means of these interrelations comes to the same thing as to grasp the events themselves. In our common-sense perceptions, too, we are not only "sensing," but also subconsciously deriving and combining. Further the concept of a multidimensional time with a finite number of dimensions again amounts to the primary concept of being. For in the case of two-dimensional time we have a complex of events situated in a $(3+2)$-dimensional space-time which *is* in the timeless sense. In order to approach in more accessible terms (with still a mental background of time flow) some vague introspective experiences, as the widening of the "now range" in a more passive state of mind, I agree with Dr. Chari that the concept of two-dimensional time may be of some use. The variation of the "now range" can then be conceived to occur in a second time, $t_2$, whereas physical time may be represented by $t_1$. Even a wider range of variation of the field of awareness can be described in this way, including extension to midlevel regions in which individuality fades. The history of variation of the field of awareness in time $t_2$ can then be represented by a part or parts of $(3+2)$-dimensional space-time bounded by a coherent or noncoherent four-dimensional surface.

However, the time $t_2$ eludes all possibilities of measurement. If we should grant to it the same status of reality as physical time, $t_1$, the bewildering situation arises that existing future events can change in time $t_2$. In this way some rare cases of warning precognitive dreams could be explained as really warning. I agree that, a priori, such a baffling situation cannot be excluded. But anybody acquainted with the prodigious imaginative faculty of the dream psyche and the distortions produced thereby will be more inclined to assume in these rare cases a dramatic deformation of the underlying precognitive sense data. Therefore I see no sufficient reason to admit

the complications inserted by the concept of multidimensional time. Furthermore, the elimination of *being beyond time* cannot be effected by introducing an infinite number of time dimensions. Such an infinite regression only shows that our common-sense conception of space and time are utterly inadequate to grasp anything more of our wider consciousness. The plain assumption that the passing of time takes time is misleading. "Time" does not pass, but is merely a human concept, presupposing a comparison of events. These events seem to me the primary stuff of reality, and only in a (not temporally) following state of mind does "time" seem to come into play.

A further major point advanced by Dr. Chari is the problem of survival. If Dr. Chari assumes that I am discarding any idea of survival of the human personality, I cannot confirm this. The question of survival of the human personality is of course bound up with the question of what "personality" (human and not human) may be. According to common sense, the consciousness of an individual develops in time, gradually arising after birth and perhaps again being extinguished at death. Already Tyrrell has emphasized in his treatise, *Apparitions,* that this simple conception of the mind does not cover the facts. Psychical research has disclosed consciousness as something baffling, demanding a drastic overhaul of all familiar convictions. It ranges from the awareness of a living body developing in time to a state of mind for which time is no barrier, at the same time extending to midlevel regions in which individuality fades. These midlevel constituents may be best described as collective states of mind representing no special bodily individual at all, but embracing different individuals, not only being the mental sum of these individual components, but also embracing the awareness of introspective differences which remain hidden for these components in their quality of pure individuality. In some phases of midlevel range it may be that only this awareness of introspective differences prevails, so that it has nothing in common with the constituting components. Beyond these midlevel regions for which neither space nor time may be barriers, consciousness seems to fade in a general background which is quite impenetrable to thought. The whole is so unfamiliar that most people are reluctant to approach, fearing any deeper introspec-

tion. If we suppose that this vast consciousness suffuses the "physical world," the lower levels of mind (akin to common sense) turn *being* into *becoming* and the higher levels again turn *becoming* into *being*.

In view of this mental hierarchy, the question of the survival of the human personality cannot be simply posed, a positive or negative answer in common sense being equally senseless. The difference between the dead, the living, and the not-yet-born appears to be only a difference in degree, vanishing the more we retire into ourselves. If Dr. Chari advances the possibility that there may be levels of consciousness in which the field of awareness can also include discarnates and in which mental contact between the incarnate and discarnate may "occur," I agree with him in the sense of "beyond time" as indicated above. Further, if we *define* survival as the ability of awareness of events beyond death (now meant in the ordinary time sequence), it seems very probable that there *is* survival. But again we have to warn against any familiar interpretation. The final confirmation of this kind of non-common-sense survival can be achieved by searching for and studying the reciprocal aspects of precognitive-retrocognitive telepathic dream complexes as indicated in my former article. This possibility of pure experimental verification seems to me of the utmost importance.

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## Do Apparitions of the Dead Imply Any Intention on the Part of the Agent? A Rejoinder to Louisa E. Rhine

### *Her Conclusion*

In this third article (**7**) of her series on "Hallucinatory Psi Experiences" (**5**, **6** and **7**) L.E.R. reached the following conclusion:

> The support once seen for survival in the "evidence" of psi hallucinations, especially those of the dying and the dead, can scarcely any longer be urged in view of present parapsychological knowledge (**7**, p. 224).

### *L.E.R.'s Hypothesis*

She says:

> The indications are, instead [of the apparition or auditory "hallucination" being evidential of survival] that the percipient generates the hallucination, agent and all, and the nature of his projection depends on his knowledge of and expectation of the agent at the time (**7**, p. 224).

### *Her Conception of the Process*

L.E.R.'s conception of the process is illustrated by her interpretation of a case in which a young woman went to sleep at the wheel while driving "up in the mountains." Her mother was praying for her "at the very moment." The girl's life was saved by hearing her mother call her name, as plainly as though the mother were beside her. L.E.R.'s hypothesis is that, since "the agent had no intention of calling to or communicating with" her daughter,

> the necessary dramatization . . . would have been as if "I am in danger. I must have my mother call to me to warn me."

### *Her Supporting Statements*

The generalizations out of which the above hypothesis was constructed deserve close scrutiny. The following appear to be the key passages:

> In both auditory and visual hallucinations [in cases in which the agent was living] numbers of instances are found in which the agent appeared to have no intent to communicate with the percipient. . . . In a number of auditory experiences the testimony of the agent was available to show that

he sometimes did and sometimes did not actually have the intention of communicating. These variations in agents' intentions were not reflected in the percipients' impressions, for even when the agent was not motivated the percipient's hallucination still pictured him as being so. . . .

The study of this group, then, showed the percipient often if not always *producing his experience according to his own interpretation of what the agent might do.* The agent's actual intent or purpose seemed not at all to be determinative, nor even in any detectable way to have been of direct influence in shaping the hallucination (**7**, pp. 205-6).

## Five Difficulties are Inherent in L.E.R.'s Position

The conclusions announced by L.E.R. are open to at least five difficulties, which appear to the present writer to be so serious as to call for systematic and dispassionate review.

### *1. She Has Failed to Meet Objections Previously Raised.*

The arguments by which L.E.R. seeks to support the above conclusion appear to the present writer to be open to almost all the major objections which have been presented in criticisms of her previous articles (**5**, **6**,) as criticized in (**1**) and (**2**). For example, her third article, which is here under discussion, still employs the question-begging term *hallucinatory,* with no attempt to meet the objection previously raised (**1**, pp. 74-75, and **2**, p. 231) that this excludes without consideration the hypothesis that apparitional bodies (at least in some cases) are self-existent structures.

### *2. Unconscious Mental Activities of the Percipient Are Crucial to L.E.R.'s Theory, but She Excludes Unconscious Mentation by the Agent.*

The "dramatization" by means of which L.E.R. explains how her percipients can perceive apparitions and clairaudient messages must obviously be a product of unconscious mentation on the part of the percipient. To explain the cases which she cites, this dramatization must make highly effective use of telepathy, clairvoyance and precognition.

But when she discusses the question (which, for her, is a crucial one) whether the agent "intended" to communicate, she takes account only of his consciously expressed intention. Moreover, she assumes that where no such intention is recorded in her cases, it did not exist.

In every place where she argues from the question of intention, the term should read: "*conscious, remembered, recorded* intention." But to describe her data thus accurately would seriously weaken, if not destroy, her argument.

*3. L.E.R. Ignores Murphy's "Field Theory."*

In October, 1945, Gardner Murphy published an article entitled "Field Theory and Survival" (**4**). In that article appeared the following sentences:

> Myers, Warcollier, Mrs. Sidgwick, and others groped toward the conception of a deep-level unity, an interindividual reality—a field, we may now say—of which all specific paranormal processes are actually aspects. . . . If, on independent grounds, there is reason to believe that the discarnate exist and are capable of contact with the living, there is no theoretical difficulty involved in their participating jointly with the living in an interpersonal psychical field (**4**, pp. 198-99).

Certainly the most mature theories hitherto advanced about apparitions (and about related phenomena) are that some sort of co-operative process is involved—some complex interaction on the unconscious levels of those involved—out of which the conscious—and sometimes the overtly and verifiably observable—phenomena emerge. Without recognizing the existence of this line of thought, and certainly without giving any reasons for dismissing it, L.E.R. treats her cases as though they were originated, developed, and consummated by individual observers, with no interaction on the part of the agent.

*4. L.E.R. Ignores Previous Findings as to Direction of Attention on the Part of the Agent.*

In the collaborative study of "Six Theories about Apparitions" (**3**, pp. 159 and 166), a statistical analysis of 165 evidentially rated cases of apparitions showed the following facts. Among 36 reported apparitions of living persons without clear evidence of full memory afterward, 69 percent were reported to have been directing special attention toward the percipient at the time. The corresponding percentage among apparitions of the living with evidence of fairly full memory afterwards was 40. Of this latter group, 92 percent appeared to some person with whom the appearer had some strong

emotional bond—such as husband, wife, betrothed, parent, child, close friend, or beloved relative. Of 104 apparitions of the dead or dying, 78 percent appeared to persons with whom they similarly had strong emotional bonds. Moreover, of the apparitions of the living who reported memories of the experience, 20 percent appeared at a time of crisis other than the appearer's own death, and 32 percent showed special concern in other ways for loved ones. The corresponding percentages among apparitions of the dead and the dying were 11 and 21.

The field psychically involved in almost all cases of apparitions of the dead, the dying, and the living thus appears to involve strong emotional bonds, quite frequently with active concern for a special crisis or special needs of the percipient. The mere conscious, remembered, and recorded "intention to communicate" is a subordinate factor in the problem, instead of being the crucial element which L.E.R.'s analysis would appear to make it.

Aside from emotional connections with the percipient, having emotional bonds with the location is a prominent factor in some types of cases. Among 38 apparitions of persons dead 12 hours or longer, 42 percent had an emotional bond with the location in which they were perceived. This fact is certainly pertinent in L.E.R.'s Example 21, and quite possibly in others.

*5. How Can a Private Hallucination, "Generated, Agent and All, by a Single Percipient," Be Perceived Consistently and Simultaneously by All Six Persons Who Were Present?*

That is exactly what was reported in L.E.R.'s Example 22. Moreover, though the percipient who reported the case had never seen even a photograph of the agent in her nurse's uniform, all six percipients so saw her—and apparently all saw her in correct perspective, as if she had been standing there. Even the family dog, who knew the agent well, "arose with his hair somewhat bristling, and growled." How *objective* can a subjective *"hallucination"* become?

This problem is explored in considerable detail in the collaborative report on "Six Theories about Apparitions," in a section entitled: "Does Collective Perception Demonstrate the Objective Re-

ality of Apparitions?" (3, pp. 201-15). L.E.R. does not *completely* ignore this difficulty. She says:

> One can then consider psi hallucinations without going into the topic of shared experiences. On any theory they would seem to be special cases (7, p. 225).

SUMMARY

L.E.R.'s conclusions (1) fail to meet objections previously raised; (2) insist upon unconscious mentation, with liberal use of telepathy, clairvoyance and precognition, on the part of her percipients, but assume that only the conscious, remembered, and reported intentions of the agents need be considered; (3) ignore highly pertinent aspects of Gardner Murphy's field theory of psychic phenomena; (4) ignore previous findings as to the major significance of direction of attention on the part of the agent; and (5) push aside the evidence and the previous discussion of collective cases which are basically inconsistent with her hypothesis.

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## Reply to Dr. Hart

In order to clarify the points on which Dr. Hart has difficulty, I will take them up in order. They are:

*1. That I neglect his hypothesis that apparitions are "self-existent structures."*

I had two reasons for not specifically considering it: First, the hypothesis concerns the agent. I was primarily studying the percipient. Therefore it was not necessary to my objective. Second, my primary objective was *to observe the role played by the percipient in the cases in my collection.* That objective did not necessitate the large order of taking into consideration all the unestablished hypotheses on the nature of apparitions. Of course if Dr. Hart's method, which, like mine, was a case study, had seemed adequate to prove his hypothesis (and in my opinion it does not), it would have been necessary for me to have taken his work into consideration. If his data had seemed to support his hypothesis sufficiently well to justify it, I would have excluded the apparitions so explained from classification as psi hallucinations. Beyond that, his conclusions would not have affected my study of the role of the percipient, for even his hypothesis does not attempt to explain all psi hallucinations as "self-existent structures."

*2. That I allow the possibility that the percipient, but not the agent, might unconsciously be the initiating influence in producing a psi hallucination.*

My objective was to see to what extent the percipient seemed to be the initiating influence. I did not *assume* "unconscious mentation" either for the percipient or agent. Instead, I found consistent evidence of it in the percipient; in the agent no consistent indications appeared. To have *assumed* it would have been to assume the point in question.

*3. That I ignore Murphy's field theory of survival.*

I fail to see why Dr. Hart should bring in this theory in this context. At any rate, I do not see that it has any essential bearing. It would apply only if, "on independent grounds, there is reason to

believe that the discarnate exist. . . ." That stage has not yet been reached.

As I stated, my objective did not require consideration of all the unproved hypotheses either of apparitions or of survival that have been advanced. I was examining psi hallucinations mainly for clues as to the initiating factors and agencies, and only partly because of the bearing of this question on survival.

*4. That I ignore Dr. Hart's findings: (a) that some agents remember being an apparition; (b) that strong emotional bonds exist between agent and percipient.*

a. The weakness of the case method for proving a point is well illustrated in this connection. The cases Dr. Hart cites, no matter how well authenticated as to facts they may have been, could not be authenticated as to interpretation. They are all open to alternatives to the one he prefers. The most commonly applicable of such alternatives is ESP, either of the telepathic or the clairvoyant type, plus the "out of the body" experience, the explanation of which is also still in question and certainly open to more than one interpretation.

In none of my cases did an agent remember having been an apparition. In a number of instances a living agent was surprised to learn he had been "seen." Cases like this clearly indicate initiative by the percipient, and give no indication that the agent had anything to do with it, consciously or unconsciously.

b. I did not ignore the important element of emotional bonds. In fact, I considered it, as Dr. Hart failed to do, against the larger background of all types of psi hallucinations. I found cases where no such bond existed; cases in which there was no interested agent; some in which there was no agent at all. I was, in short, observing in unselected material the indications nature gives instead of in the selected material that has too often been over-interpreted as supporting a favored hypothesis.

*5. That I disregard the evidence from joint hallucinations.*

(Evidence that the apparition must, because jointly seen, be a "self-existent structure," is, I think, the implication here.) I do not disregard that evidence, but I consider it inconclusive, just as the

case method is inconclusive, for counterhypotheses such as illusion, suggestion, and telepathy cannot be excluded.

I think we can agree that we have too few good cases of joint hallucination on record to justify any attitude but that of suspended judgment. The cases we do have are potentially so important, however, and so puzzling that, as I indicated in my article, they deserve to be kept in the line of investigation. Their explanation should be significant whether it turns out to be within the bounds of hallucinatory mental action, or whether they really ever do in any case represent "self-existent structures."

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## BOOK REVIEW

LES GRANDS MEDIUMS. By Robert Amadou. Paris: Editions Denoël, 1957. 259 pp.

This book is a short history of the great physical mediums of the last century—about twenty of those well-known personalities whose work received wide attention, who allowed themselves to be observed and investigated, and whose counterparts, the author reminds us, do not exist today. The twenty are: Angélique Cottin, the Fox sisters, D. D. Home, Florence Cook, Stainton Moses, Mme. Blavatsky, Kathleen Goligher, Eusapia Palladino, Marthe Béraud, Ladislas Lasslo, Pasquale Erto, Ejner Nielsen, Stella C., Margery Crandon, Stanislawa P., Frank Kluski, Jean Guzik, and the Schneider brothers.

The author's purpose was to collect available testimony about the work of these mediums from the accounts of witnesses, from investigations, and from other relevant sources, both favorable and unfavorable, and to bring all this material to bear on one question: Is there scientific evidence of genuine phenomena? It was not to be expected that he would end up with an unequivocal answer. When the obvious frauds and the apparent successes were summed up and evaluated by the author, his conclusion, in fact, was: No one can affirm that the paranormal physical effects attributed to the mediums are not genuine. But if we preserve the scientific exactitude proper to parapsychology, no one can affirm their authenticity either.

Those (like the writer of this review) who are not thoroughly acquainted with the history of mediumship can be grateful to the author for having done a great deal of research and rounded it up into a concise, well-written book. In French, its use will be somewhat curtailed; we must hope for an English translation.

DOROTHY H. POPE

# PARAPSYCHOLOGICAL ABSTRACTS

SARA R. FEATHER

[This section consists of abstracts of reports published in other journals and of unpublished reports filed at the Parapsychology Laboratory of Duke University. Readers may obtain unabridged copies (Thermo-Fax) of the unpublished reports by writing to the Journal of Parapsychology, College Station, Durham, N. C. The price is ten cents per page.]

DUCASSE, C. J. Physical phenomena in psychical research. *J. Amer. Soc. psych. Res.*, 1958, **52**, 3-23.

As a preface to his examination of parapsychical phenomena, Ducasse analyzes the reasons why this area of psychic research has been neglected in recent years. In addition to stressing the rarity of mediums in whose presence physical phenomena are reported to occur and the apparent difficulty of meeting the desire for repeatability, he belabors those who seem to have a will to disbelieve and who tend to dismiss all reported physical phenomena as fraudulent because of detection of some fraud in this area. He believes it logically and psychologically naïve to ignore all the work of a medium believed to have been detected in fraud only in certain areas. The parapsychical occurrences he reviews include reports of raps, poltergeist phenomena, levitations, telekinesis, fire immunity, materializations and dematerializations. If these phenomena are genuine, the scientific implications are momentous. Ducasse maintains that the hypothesis of spirit agency has relevance to parapsychical physical phenomena only when intelligence and purpose are manifested, but that this type of phenomena, however striking, does not constitute particularly strong evidence of survival of the human mind after death. He emphasizes the need for explicit formulation of the criteria, if any, upon which one might distinguish between a materialization that would be evidence of survival and one that is only a memory image in one's mind. He concludes with a plea for genuine objectivity of judgment in the investigation of reported parapsychical physical phenomena.

FAIRFIELD, L. Thérèse Neumann. *J. Soc. psych. Res.*, 1957, **39**, 164-73.

The salient occurrences and claims concerning the life history and reportedly parapsychical phenomena of Fraulein Neumann are summarized and subjected to critical examination. The difficulties present in ascertaining the facts about Thérèse Neumann are discussed and it is noted that the extraordinary allegations regarding this case have never been subjected to adequate test.

FISK, G. W., and WEST, D. J. Psychokinetic experiments with a single subject. *Newsletter of the Parapsychology Foundation.* Vol. 4. No. 6, Nov.-Dec., 1957.

A résumé is given of five experiments, conducted over a six-year period, which appear to indicate a persistent psychokinetic effect associated with a particular individual, Dr. J. Blundun. The first three experiments have already been reported, and this paper deals with the last two.

Throughout the entire series, Dr. Blundun made daily cup-casts for a succession of target faces unknown to her which were either (1) selected and displayed by Fisk; (2) alternately selected and displayed by Fisk and West; or (3) sealed and sent to the subject. In all but the first experiment, the order of the target faces was determined randomly and the face frequencies were equalized.

Excluding her first 10,000 trials (on the basis of which she was selected for further testing), Dr. Blundun completed 15,032 trials with a deviation of 172 from mean chance expectation ($P = .00017$). The significance of the results appears to be increased by the application of a differential method of scoring, the Die Orientation Method, developed by A. M. J. Mitchell.

In the fourth experiment, Fisk and West alternated in displaying the target for six days each (with Dr. Blundun assuming that all targets were being displayed by Fisk, as before). Her total scores on Fisk's targets were significant ($P = .007$), whereas those obtained when West handled the targets were at the chance level.

Suggestive results were obtained when Fisk employed the 3600 trials of the last experiment as a basis for predicting the die-face

which each sealed target envelope contained (the envelope having been prepared by an assistant).

FISK, G. W., and WEST, D. J. Towards accurate predictions from ESP data. *J. Soc. psych. Res.*, 1957, **39**, 157-62.

An unsuccessful attempt was made to predict the contents of a set of sealed target cards on the basis of the majority guess for each card by a group of subjects in a distance clairvoyance test. Fourteen subjects, selected because of previously demonstrated ESP ability, made daily guesses of the order of a set of twelve clock-face cards, one card of each number from 1 to 12. On each day of the experiment D.J.W. shuffled the target cards, which had been placed in opaque envelopes by a third person, and displayed them in a row at his home.

A total of 3696 calls were made over an eleven-week period, and, in addition, 1332 calls were made during the last seven weeks on targets which were visible to the experimenter D.J.W.

Although analysis failed to reveal a single correspondence between the "majority votes" on the sealed targets and the actual contents of the envelopes, a chi-square evaluation of the 12-by-12 table of calls in relation to target envelopes indicated an unequal distribution of preferences that was marginally significant. Chi-square analysis of the distribution of guesses on closed and open targets taken together gave a significant result, demonstrating a nonrandom association between targets and calls, which the authors consider evidence of "misguided" ESP.

MICHIE, D., and WEST, D. J. A mass ESP test using television. *J. Soc. psych. Res.*, 1957, **39**, 113-34.

No evidence of a generalized ESP effect was obtained in a mass GESP test conducted over television with G. W. Fisk acting as agent and the audience as subjects. However, one entrant, Mr. B. Downey, obtained results that were considered worthy of further investigation. Satisfactory statistical evidence of an ESP effect was obtained in a series of five subsequent GESP tests with Mr. Downey guessing the order of clock-face cards. Analysis by a "divergence" system of scoring the clock cards revealed a highly significant dif-

ference ($P < .001$) between Downey's results when Fisk was agent and when D. J. West was agent. The scoring with Fisk was very high ($P < .00001$), while that with West was slightly negative. A method is described for dealing with the sequence and position preference of a large number of subjects guessing the same set of targets.

MURPHY, GARDNER. Triumphs and defeats in the study of mediumship. *J. Amer. Soc. psych. Res.*, 1957, **51**, 125-35.

A brief review of the rise of mediumship and the work of the notable Boston medium, Mrs. Piper, in particular, is presented as a background to an examination of the question: Can mediumistic communications be used to prove the survival of deceased human beings and their continuing capacity to communicate? Murphy attempts to show that this question is improperly formulated and unanswerable.

Large quantities of communicator-like material have been produced by mediums, but there have also been gross mistakes. Perhaps the most realistic hypothesis, which was suggested over fifty years ago by Mrs. Sidgwick, is that part of the phenomena (e. g., the "controls") originate in the medium's own mind, whereas the evidential material may be the result of telepathy from the deceased or from the living. Difficulties arise in the attempt to determine the source of the evidential material, because of the considerable evidence of entranced sensitives having made telepathic contact with distant, as well as present, living persons. Some attempt to circumvent this difficulty has been made by conducting proxy sittings where a sitter asks for information about distant persons unknown to the medium and preferably to the sitter. In these cases, the psychological bond between the actual sitter and the medium is relatively weaker than in regular sittings.

Murphy believes that the strongest survival evidence from other than spontaneous cases comes from mediumistic communications in which the message appears to represent an integrated shared bundle of information (cross-correspondences) from two deceased personalities. However, he notes the possibility that some living persons

may still possess the information involved, even in that particular interlocked way.

Yet another possibility in approaching this problem is that there may be some aspects of personality which cannot be considered on a time dimension, so that the whole question of continuity after death is wrongly stated. But Murphy concludes that mediumship, notably the cross-correspondence work, has provided some of the most extraordinary and best documented instances of parapsychical powers ever recorded and more than any other method of psychical research has been a "very rich avenue of information into the psychology of the paranormal processes."

NICOL, J. F., and NICOL, B. H. Investigation of a curious "hunch." *J. Soc. psych. Res.*, 1958, **52**, 24-34.

Corroborative statements were obtained from the two principal persons involved in a spontaneous case of apparent ESP.

VAN DE CASTLE, R. L. Differential patterns of ESP scoring as a function of differential attitudes toward ESP. *J. Soc. psych. Res.*, 1957, **51**, 43-61.

Subjects from seven different experimental groups were classified into four categories (high sheep, sheep, conflict, and goat) on the basis of their responses before the ESP tests to a series of 10 incomplete sentences concerning attitude toward ESP. These four attitudinal groups did not differ significantly in the mean number of hits per subject obtained in the eight-run clairvoyance tests, but significant inter-group differences were found (1) in the variation of the individual's scoring from run to run; and (2) in the variation (between subjects) of the total number of hits per subject. Subjects in the two sheep groups had very similar scores throughout their eight runs (exhibiting little run-to-run variation), but differed significantly with regard to between-subject variance. Similarly, the conflict and goat subjects were very similar regarding their within-subject variance (exhibiting considerable run-to-run variation), but differed significantly in their between-subject variance.

ZORAB, G. ESP experiments with psychotics. *J. Soc. psych. Res.*, 1839, **39**, 162-64.

Following a pilot test (which gave only chance results), an ESP experiment consisting of 11,000 clairvoyance trials was conducted with 16 psychotic patients at a large Dutch psychiatric clinic. The BT technique of card guessing was employed (the card being laid aside by the experimenter after each call), and the experimenter and subject were separated by a screen. No significant results were found either in the series as a whole or in the scoring of any of the three patient groups: the schizophrenics, the manic-depressives, or those classified as having "various mental disorders."

# THE PARAPSYCHOLOGICAL ASSOCIATION

## First Annual Convention

The first annual convention of the Parapsychological Association will be held at City College, New York, N. Y. from Thursday, September 4, through Saturday, September 6, 1958. Further information will be announced in the June *Journal*.

## Additional Announcements

The Council of the Parapsychological Association would like to call attention to the following change which has been made in the Constitution. The date in Article V, Section 7 of the Constitution has been altered and now reads: "Persons who apply for membership in the Association on or before June 19, 1958, as Members or Associates and are elected in due course shall be designated as Charter Members and Charter Associates."

The Membership Committee announces that to date the Association has 36 Members and 29 Associates.

# GLOSSARY

A description of the basic experimental methods, of the findings, and of the statistical procedures for evaluating ESP and PK results may be found in *Parapsychology, Frontier Science of the Mind* by J. B. Rhine and J. G. Pratt (published by Charles C. Thomas, Springfield, Illinois, U.S.A., Blackwell Scientific Publications, Ltd., Oxford, England, and The Ryerson Press, Toronto, Canada).

AGENT: The "sender" in tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

BT (Basic Technique): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

*Chance Expectation = Mean Chance Expectation:* The most likely score if only chance variation is present.

*Chance Average:* Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic cognition of the mental activities of another person.

CR (Critical Ratio): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearsons.)

$CR_d$ (Critical Ratio of the Difference): The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

*Backward Displacement:* ESP responses to targets preceding the intended targets. Displacement to the targets one, two, three, etc. places preceding the intended target are designated as (—1), (—2), (—3), etc.

*Forward Displacement:* ESP responses to targets coming later than the intended targets. Displacement to the targets one, two, three, etc. places after the intended target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the pack before any are removed or checked.

ESP (Extrasensory Perception): Awareness of or response to an external event or influence not apprehended by sensory means.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, square, cross and waves (three parallel wavy lines).

ESP PACK: Twenty-five ESP cards.

*Closed Pack:* An ESP pack composed of five each of the five symbols.

*Open Pack:* An ESP pack made up of the ESP symbols selected in random order.

EXPECTATION: See *Chance.*

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

GESP (General Extrasensory Perception): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

MCE (Mean Chance Expectation): see *Chance.*

P (Probability): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with behavioral or personal effects that are demonstrably nonphysical (that is, which do not fall within the scope of known physical principles).

PERCIPIENT = SUBJECT: The person who makes the calls in an ESP test.

PK (Psychokinesis): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PLACEMENT TEST: A PK technique in which the aim of the subject is to try to influence falling objects to come to rest in a designated part of the throwing surface.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring responses to free material. A judge ranks the stimulus objects (usually pictures in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes which are nonphysical in nature. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI-MISSING: Exercise of psi ability in a way that avoids the target the subject is attempting to hit.

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

PSYCHICAL RESEARCH: Older term used for parapsychology.

QD (Quarter Distribution): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RANDOM ORDER: An order of events which displays no trends or regularities that would allow any inference regarding one event from one or more of the others in the series.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in any given unit of trials.
*Total Score:* Total of scores made in a given number of runs.
*Average Score:* Total score divided by number of runs.

SD (Standard Deviation): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$ in which $n$ is the number of single trials, $p$ the probability of success per trial, and $q$ the probability of failure.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in parapsychology today is a probability value of .01 or less, or a deviation in either direction such that the CR is 2.58 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

SPONTANEOUS PSI EXPERIENCE: Natural, unplanned occurrence of an event or experience that seems to involve parapsychical ability.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

STM (Screened Touch Matching): An ESP card test in which the subject indicates in each trial (by pointing to one of five key positions) what he thinks the top card is in the inverted pack held by the experimenter behind a screen. The card is then laid opposite that position.

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET: In ESP (including precognition) the objective or mental events the subject is attempting to respond to. In PK tests, the faces of the die which the subject attempts to bring up in the act of throwing. In placement tests, the designated part of the throwing surface.

*Target Card:* The card which the percipient is attempting to identify or otherwise indicate a knowledge of.

*Target Pack:* The pack of cards the order of which the subject is attempting to identify.

*Target Face:* The face on the die which the subject tries to turn up by PK.

TELEPATHY: Extrasensory perception of the mental activities of another person (as distinguished from clairvoyant cognition of objective events).

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.